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76

The European Witch Craze of the 14th to 17th Centuries
—Ben-Yehuda

Class and Organization in the Parisian Insurrection of June 1848
—Traugott

Communication via Mutual Gaze—Mazur, Rosa, Faupel,
Heller, Leen, and Thurn

Divorce: An Expansion of Becker's Theory of Marital Instability
—Huber and Spiro

Property Crime Rates in the United States, 1947-1977
—Cohen, Felson, and Leven

Metropolitan Morphology and Population Mobility
—Sly and Tayman

RESEARCH NOTES

The Deterrent Effect of Capital Punishment—Phillips
A Formal Theory of Selection for Schizophrenia—Eaton

Review Essay on Simmel—Lawrence
Review Essay on Matthes et al.—Marshall

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CONTENTS

- CUC - H02831-76 - P0095831*
1003
- 1 The European Witch Craze of the 14th to 17th Centuries: A Sociologist's Perspective
NACHMAN BEN-YEHUDA
 - 32 Determinants of Political Orientation: Class and Organization in the Parisian Insurrection of June 1848
MARK TRAUGOTT
 - 50 Physiological Aspects of Communication via Mutual Gaze
ALLAN MAZUR, EUGENE ROSA, MARK FAUPEL, JOSHUA HELLER, RUSSELL LEEN, AND BLAKE THURMAN
 - 75 Considering Divorce: An Expansion of Becker's Theory of Marital Instability
JOAN HUBER AND GLENNA SPITZE
 - 90 Property Crime Rates in the United States: A Macrodynamic Analysis, 1947-1977; with Ex Ante Forecasts for the Mid-1980s
LAWRENCE E. COHEN, MARCUS FELSON, AND KENNETH C. LAND
 - 119 Metropolitan Morphology and Population Mobility: The Theory of Ecological Expansion Reexamined
DAVID F. SLY AND JEFFREY TAYMAN

Research Notes

- 139 The Deterrent Effect of Capital Punishment: New Evidence on an Old Controversy
DAVID P. PHILLIPS
- 149 A Formal Theory of Selection for Schizophrenia
WILLIAM W. EATON

Commentary and Debate

- 159 The "Common Structure Semigroup," a Replacement for the Boorman and White "Joint Reduction"
PHILLIP BONACICH
- 166 Algebraic Approaches to the Comparison of Concrete Social Structures Represented as Networks: Reply to Bonacich
SCOTT A. BOORMAN AND PHIPPS ARABIE

175 Concerning Another Response
HARRISON C. WHITE

175 Sex, Segments, and Stratification: Comment on Snyder, Hayward,
and Hudis
JAMES N. BARON

Review Essays

182 Radicalism and the Cash Nexus
PETER A. LAWRENCE

187 From Periphery Back to Center
DAVID MARTIN

Book Reviews

192 *What Is Sociology?* by Norbert Elias. Translated by S. Mennell and
G. Morrissey

Human Figurations: Essays for Norbert Elias edited by Peter R.
Gleichmann, Johan Goudsblom, and Hermann Korte
LEWIS A. COSER

194 *The Essential Tension: Selected Studies in Scientific Tradition and
Change* by Thomas S. Kuhn
THOMAS F. GIERYN

198 *Sociology and the Twilight of Man: Homocentrism and Discourse in
Sociological Theory* by Charles C. Lemert
DON MARTINDALE

200 *The Derelicts of Company K: A Sociological Study of Demoraliza-
tion* by Tamotsu Shibutani
KURT LANG

202 *Military Institutions and Coercion in the Developing Nations* by
Morris Janowitz
GWYN HARRIES-JENKINS

204 *Academic Power in Italy: Bureaucracy and Oligarchy in a National
University System* by Burton R. Clark
ROBERTA GARNER

206 *Churches and Church Goers: Patterns of Church Growth in the Brit-
ish Isles since 1700* by Robert Currie, Alan Gilbert, and Lee Horsley
N. J. DEMERATH III

208 *Marginal Workers, Marginal Jobs: The Underutilization of Ameri-
can Workers* by Teresa A. Sullivan
CHARLES HIRSCHMAN

211 *Social Influence and Social Change* by Serge Moscovici. Translated
by Carol Sherrard and Greta Heinz
VERNON L. ALLEN

213 *Policing the Crisis: Mugging, the State, and Law and Order* by Stuart
Hall, Chas Critcher, Tony Jefferson, John Clarke, and Brian Roberts
AUSTIN T. TURK

10 9543

- 214 *Swindling and Selling* by Arthur Allen Leff
PETER K. MANNING
- 217 *Odd Jobs: The World of Deviant Work: Confidence Men, Fences, Fortune Tellers, Strippers, Medical Quacks, Prostitutes, Female Impersonators, Bookmakers, Racketeers, Safecrackers* by Gale Miller
JOSEPH HARRY
- 218 *On the Take: From Petty Crooks to Presidents* by William J. Chambliss
JOHN R. HEPBURN
- 220 *Morals and Markets: The Development of Life Insurance in the United States* by Viviana A. Rotman Zelizer
CAROL A. HEIMER
- 223 *The Ways Out: Utopian Communal Groups in an Age of Babylon* by John R. Hall
RICHARD OFSHE
- 225 *The Children of Prosperity: Thirteen Modern American Communes* by Hugh Gardner
LEIGH MINTURN
- 227 *Sex Roles, Life Styles and Childbearing: Changing Patterns in Marriage and the Family* by John H. Scanlon
Illegitimacy, Sexuality and the Status of Women by Derek Gill
LYNN SMITH-LOVIN
- 231 *The Africanization of the Labor Market: Educational and Occupational Segmentation in the Cameroun* by Remi Clignet
JAMES A. M. ELLIOTT
- 234 *The Social Bond: An Investigation into the Bases of Law-Abidingness*. Vol. 1: *Antecedents of the Social Bond: The Phylogeny of Sociality*. Vol. 2: *Antecedents of the Social Bond: The Ontogeny of Sociality* by Werner Stark
FORREST DILL
- 236 *Cultural Conceptions and Mental Illness: A Comparison of Germany and America* by John Marshall Townsend
GEORGE BECKER
- 238 *So Far Disordered in Mind: Insanity in California, 1870-1930* by Richard W. Fox
R. A. WHITFIELD
- 240 *Race Relations in Chicago: Second Survey: 1975* by George Surgeon, Judith Mayo, and Donald J. Bogue
MORTON WEINFELD
- 243 *Images of Law* by Zenon Bankowski and Geoff Mungham
PHILIP S. C. LEWIS
- 244 *Loners, Losers, and Lovers: Elderly Tenants in a Slum Hotel* by Joyce Stephens
DANIEL R. KRAUSE

IN THIS ISSUE

NACHMAN BEN-YEHUDA is a lecturer in the Department of Sociology, Hebrew University of Jerusalem. His interests include witchcraft, modern occultism (and the esoteric culture), science fiction, deviant science, deviance theory (and drug misuse in particular), and social psychology.

MARK TRAUGOTT is assistant professor of sociology at the University of California, Santa Cruz. He has previously published a volume of translations of and commentary on Durkheim (*Emile Durkheim on Institutional Analysis*) and in the area of social movements. He is currently working on a book-length manuscript on the Revolution of 1848 in France extending the work appearing in this issue. His next project will also be in historical sociology, specifically on the end of the Second French Republic and the beginning of the Second Empire.

ALLAN MAZUR is professor of sociology at Syracuse University. His research specialties are biosociology and the sociology of science and technology. His book, *The Dynamics of Technical Controversies*, will be published this year.

EUGENE ROSA is assistant professor of sociology at Washington State University. His major research interests are in (1) the biosociological aspect of face-to-face behavior and (2) the emergent field of energy studies.

MARK FAUPEL is a doctoral candidate in the Department of Psychology at the University of Georgia.

JOSHUA HELLER is a graduate student in sociology at Syracuse University. His interests include social psychology, the sociology of social conflicts, and visual sociology.

RUSSELL LEEN expects to receive his Ph.D. in anthropology from Syracuse University in 1980. His dissertation is entitled "The Social and Economic Impacts of Nuclear Energy."

BLAKE THURMAN is assistant professor of anthropology at Hobart and William Smith Colleges.

JOAN HUBER, professor of sociology at the University of Illinois at Urbana-Champaign, is exploring the historical relationship of aggregate changes in fertility and women's labor force participation to beliefs about appropriate sex-role behavior.

GLENNA SPITZE is assistant professor of sociology at the State University of New York at Albany. Her current research concerns the social bases of sex-role ideologies (with Joan Huber) and the relations between female labor force participation and family migration.

LAWRENCE E. COHEN is associate professor of sociology at the University of Texas at Austin. His research centers on the analysis of victimization survey results and forecasting crime rate trends.

MARCUS FELSON, associate professor at the University of Illinois at Urbana-Champaign, specializes in human chronography. This is the study of the tempos, rhythms, and timing of human collaborative, competitive, and predatory activities and of related social trends. His current research involves classifying various social and environmental phenomena by hour of day and studying how the dovetailing of activities sets the stage for various social problems and benefits which result by systematic accident.

KENNETH C. LAND is professor of sociology and director of the Social Science Quantitative Laboratory at the University of Illinois at Urbana-Champaign. He is currently engaged in the development of macrodynamic models of social changes in the post-World War II United States. His other work pertains to the development of models and methods in mathematical sociology and social statistics.

DAVID F. SLY, associate professor of sociology at Florida State University, Tallahassee, is currently engaged in research on population redistribution and personal energy use with Wilbur Zelinsky and in a panel study dealing with the formation and transformation of migration expectations in Kenya.

JEFFREY TAYMAN received his Ph.D. from Florida State University in December 1979 and is a research investigator in the State of Washington's Population, Enrollment and Economic Studies Division. His research interests lie in the areas of demographic techniques, human ecology, and migration, and he is currently working on several projects related to population estimation and forecasting techniques.

DAVID P. PHILLIPS is associate professor of sociology at the University of California, San Diego. He is currently completing a study of the effects of capital punishment on homicide in America. He is also studying suicide, suggestion, and the impact of the mass media on violence.

WILLIAM W. EATON is assistant chief, Center for Epidemiologic Studies, National Institute of Mental Health, and has a Ph.D. in sociology from the University of Wisconsin. His most recent publication is *The Sociology of Mental Disorders*.

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The European Witch Craze of the 14th to 17th Centuries: A Sociologist's Perspective¹

Nachman Ben-Yehuda

Hebrew University of Jerusalem

From the early decades of the 14th century until 1650, continental Europeans executed between 200,000 and 500,000 witches, 85% or more of whom were women. The character and timing of these executions and the persecutions which preceded them were determined in part by changed objectives of the Inquisition, as well as by a differentiation process within medieval society. The witch craze answered the need for a redefinition of moral boundaries, as a result of the profound changes in the medieval social order. The fact that these executions and the accompanying demonological theories enjoyed widespread and popular acceptance can be explained through the anomie which permeated society at that time. While these conditions provided the intellectual, cognitive background for the witch-hunts, economic and demographic changes, together with the emotional need for a target, explain why the witch-hunts were directed at women.

The continental European witch craze, in its most virulent form, lasted from the early decades of the 14th century until 1650. This paper attempts to analyze this phenomenon from a macrosociological point of view, concentrating on several questions clustered along three axes. The first axis concerns timing: Why did the witch craze start in the 14th century? Why did it become a popular and widespread craze between the 15th and 17th centuries? Why did it end in the 17th century? The second axis pertains to content: Why the suddenly increased attention to witchcraft, black magic, and the like? How can the emergence of a whole religious ideology concerning the witches, conceived as an antithesis to "true" Christianity,

¹ I am indebted to S. N. Eisenstadt, Morris Janowitz, Edward Shils, Victor Azaria, Barbie Zelizer, and three anonymous readers whose comments on a previous draft were tremendously helpful in revising and refining the arguments presented here. In particular, I would like to express my deepest gratitude to Barry Schwartz for his encouragement, patience, and penetrating criticism throughout the lengthy process of researching and writing this paper and to Joseph Ben-David for his patience and for innumerable and indispensable insightful comments and suggestions. I am also very grateful for the Blitsteins' warm support, without which this paper would never have materialized. Grants from the Department of Sociology, University of Chicago, and the Faculty of Social Sciences, Hebrew University, enabled the writing of this research report.

be explained? Why did this ideology culminate in persecutions of witches? How can we explain the use of witchcraft—an age-old phenomenon—for faking of an antinomian theology, not by those who adhered to it, but by those supposedly opposed to it? The third axis involves the target of the witch-hunts: Why were women singled out as the main victims?

The answers suggested are based on the convergence of various conditions. The vested interests of such control organs of the Catholic church as the Dominicans and the Inquisition, and the collapse of the authoritative framework of religion and of the feudal social order, offer answers related to the first axis. The dissolution of the medieval cognitive map of the world, which gave rise to utopian expectations, magical beliefs, and bold scientific explorations, relates to questions along the second axis. Changes in the economy, demography, and family, especially changes in the role of women—some of which were of catastrophic proportions—explain the nature of the target of the craze. The spatial distribution of the witch-hunt and its termination resulted respectively from the presence or absence of all or some of these conditions in different parts of Europe during the period in question and from their disappearance everywhere at the end of the period.

Before I attempt to deal with these explanations, it is important to describe the particular characteristics of the European witch craze itself and to determine exactly which of its aspects require explanation.

WITCHCRAFT, WITCH-HUNTS, AND WITCH CRAZE: THE PHENOMENON TO BE EXPLAINED

Although the canonical books of the Old Testament nearly ignore the subject, one of the few places where witches are mentioned is in the story of the "Witch of Endor" with whom King Saul consulted before his last battle (1 Sam. 28:9). A second example is embedded in the Law itself, in the divine command "Thou shalt not suffer a witch to live" (Exod. 22:10; Williams 1959, p. 27). Throughout the Bible, however, all stories concerning witches are neutral in the sense that witches, devils, and demons are never elaborately conceptualized, and the existence of an all-encompassing supernatural, demonic world is never mentioned.

According to leading authorities on magic in classical Greece and Rome, magical processes were employed in both societies to produce rain, prevent hailstorms, drive away clouds, increase wealth and the like, but were also used for evil purposes (Baroja 1965; Hughes 1965).

O'Dea (1966) defines religion as "... the manipulation of non-empirical or supra-empirical means for non-empirical, or supra-empirical, ends. ..." In contrast, he defines magic as "... the manipulation of non-empirical or supra-empirical means for empirical ends ..." (O'Dea 1966,

p. 7). Such an approach recalls the work of Weber (1964), for whom witchcraft is a kind of technology and the magician's main function is to cope with relatively ad hoc interests and tensions. Magical powers can be "forced" to serve human needs through the magician's correct use of formulas.

These, I believe, were the essential characteristics of European witchcraft practice before the witch-hunts of the 14th to 17th centuries: its "technological" nature, its ad hoc purposes, its extremely specific goals (love potions, specific spells, love magic, and the like). There did not yet exist a developed, systematic conceptualization of a negative supernatural world, diametrically opposed to this world and at war with it. The witch, so far, had a special position vis-à-vis the gods (or deities): with the correct "technological" use of spells, potions, and the like, she could compel them to perform specific actions.

In the European witch-hunts of the 14th to 17th centuries, however, witchcraft was transformed into a completely evil entity which created problems instead of solving them. With the publication of the *Malleus maleficarum* [The witch's hammer] in the 1480s, demonological theories reached a peak in which witchcraft constituted an independent "antireligion." The witch lost her powerful position vis-à-vis the deities: her ability to force the deities to comply with her wishes was replaced by a total subordination to the devil. In short, the witch became Satan's puppet.

These changes in the conceptualization of witchcraft are of crucial importance. Because witchcraft was regarded as a routine, day-to-day (almost personal) technology until the 14th century, witches were classified as good or bad, depending on the objective of their magic. After the 14th century, a whole systematic theory was devoted to witchcraft: books were written on the subject, and experts specialized in its theory ("demonologists") and practice ("inquisitors," "witch-hunters," and the like). This analytical shift to the "new" eclectic demonological theories was precisely what was needed to enable the inquisitors, and other individuals, to persecute legitimately hundreds of thousands of witches.

The witch-hunts did not affect all the areas of continental Europe in the same way. Also, the English witch craze was notably different from that in continental Europe (see Anderson and Gordon 1978; Currie 1968; even though both are poorly argued; see also Keith 1971; Notestein 1968; MacFarlane 1970). The witch craze in Scotland, on the other hand, highly resembled the continental pattern (Robbins 1959).² From the

² In England the witch craze started and ended later than in continental Europe and was much milder. There, a demonological ideology did not prevail and persons accused of witchcraft were considered to have committed crimes against men and not against God. It is very likely that the lack of inquisitorial machinery, the clear-cut relationships between church and kingdom, and a strong monarchy rendered less painful the English struggle with the problems Europe was faced with. Furthermore, in England

sources available, it appears that the worst European witch-hunts occurred in Germany, Switzerland, and France, and that those in other areas were far less extreme (Cohn 1975; Hughes 1965; Kieckhefer 1976; Lea 1957; Monter 1969, 1976; Robbins 1959; Russel 1972; Trevor-Roper 1967; Williams 1959; Midelfort 1972).

In general the European witch craze began to crystallize in the second and third decades of the 14th century. A fervent believer in the power and reality of magic, Pope John XXII himself gave it its impetus as he encouraged inquisitors to persecute relentlessly all sorcerers, magicians, and other heretics owing to his fear that witchcraft practices were rapidly spreading (Lea 1901, 3:450-54). In effect, however (Lea 1901), the actions of Pope John XXII merely reinforced already growing fears about witchcraft. In 1326 he issued his *Super illius specula* which "authorized the full use of inquisitorial procedure against witches" (Trevor-Roper 1967, p. 103). There he specifically stated that "... some people, Christian in name only, have forsaken the first light of truth to ally themselves with death and traffic with hell. They sacrifice to and adore devils; they make or obtain figurines, rings, vials, mirrors . . . by which they command demons . . . asking their aid . . . [and] . . . giving themselves to the most shameful subjection for the most shameful of ends . . ." (Robbins 1959, p. 288).

The pope's efforts resulted in a small scale witch-hunt in the Alps and the Pyrenees lasting for more than a century and a half. Although individual, scattered trials of witches had already been carried out in 1245 and 1275 (Robbins 1959, pp. 207-9, 287-88), the early decades of the 14th century witnessed a tremendous intensification of attempts to stifle witchcraft practices, especially in France. The witch craze continued to gain momentum throughout the 14th and 15th centuries, and by the 1490s the hunts were being conducted in full fervor. No doubt they were accelerated by the publication of the *Malleus maleficarum* by Dominicans Sprenger and Kramer in 1487-89 (reprint 1968) (Zilboorg and Henry 1941). "... European witchcraft . . . between 1450 and 1750 . . . was conceived of as a virulent and dangerous blend of sorcery and heresy. Sorcery [includes] anything that aims at negative supernatural effects through formulas and rituals. The other element, heresy . . . is the pact with the Devil, the Witches' Sabbath in the form of a black or inverted Mass . . ." (Monter 1969, p. vii).

The main feature of the European witch craze was the "Witches' Sab-

the judicial system was more humane than in Europe. It appears that during the 14th to 17th centuries England was in many respects outside the dispute which tore Europe apart. Scotland, however, experienced much more religious turmoil, which affected the judicial foundations of the law and—together with King James VI of Scotland's personal encouragement—enabled the occurrence of a virulent witch craze there.

bath," the climax of which was a huge orgy between the devil and witches; at this time new witches were initiated. The ceremony allegedly included denying salvation, kissing the devil's posterior, spitting on the Bible, having promiscuous sexual orgies, feasting on roasted or boiled unbaptized children's flesh and exhumed corpses, mocking the holy sacrament of baptism, cursing the cross, and the like (Mackay [1841] 1932; Murray 1962; Carus 1974; Trevor-Roper 1967; Russell 1977).

Such a negative view of the witch, as well as the whole new perception of witchcraft and demonology, dramatically changed the perception of witchcraft as technology: witchcraft was losing its neutral technological character in favor of an elaborated and complicated image of an antireligion. The total ideological transformation in the perception of the witch, however, did not take place until the 15th century, when it was crystallized, authorized, and accepted.

The Dominicans' theories concerning witchcraft were widely accepted at the time. They based their beliefs on a dualistic assumption which viewed the world as a battlefield in which a struggle between the godly sons of light and the satanic sons of darkness was being played out. The stories and myth of the witches can be regarded as the exact qualitative opposite of the conception of Christ, and witchcraft as the exact opposite of what was supposed to be the true faith, Christianity (Ben-Yehuda 1976, pp. 94-99).

In opposition to the idea of the holy birth of Jesus, the Dominicans tell us of a perverse and barren sexual intercourse between the devil and the witch. We are told further that the devil, appearing as either an attractive, supersexual female form ("succubus") or a supersexual male form ("incubus"), would come before a human male or female, respectively, in order to seduce them. However, because the incubus did not possess his own sperm, the human female had to steal it from her unsuspecting husband in order to copulate with the devil. There were also many reports that the succubus was in fact a revived corpse, who became a corpse again³ once her identity was discovered (see, e.g., Robbins 1959, pp. 254-59, 461-69, 490-92).

Contrary to the day when Christians meet to pray—Sunday morning—the devil and his legions prefer the night between Friday and Saturday. Christians meet in a holy church; the devil and his legions in weird places such as cemeteries. In the church, people have the holy images of Jesus

³ The explicit sexual overtones of the witch-craze myth cannot—and should not—be ignored. Demonologists went to great lengths to associate witchcraft with detailed descriptions of perverted sexual practices and with the overt, irresistible seductive sexual behavior of the devil's legions. Emphasis was laid particularly on the "insatiable" sexual appetite of women: incubi, for example, outnumbered succubi by at least 10 to one (Robbins 1959, p. 490).

and Mary to revere; at the witches' Sabbath, the images are of toads, stinking goats, cats, dogs, and the like. In church, people kiss the cross; at the Sabbath, they kiss the he-goat's posterior. The symbols and objects used at the ceremony in the church (wine, wafers, water) are mocked at the Sabbath. In contrast to the holy baptism, the devil had his own—a mark imprinted on the witch, while filthy water was sprinkled throughout the ceremony by stinking toads. Music was also played during the satanic ceremonies; however, in contrast to the music played in church, this music was macabre, played with strange instruments like horses' skulls, oak logs, human bones, and the like. In church, people tasted the holy symbols (wafers and wine); at the Sabbath they feasted on the roasted flesh of unbaptized babies (MacKay [1841] 1932; Trevor-Roper 1967). All in all, the overwhelming evidence indicates that the witch beliefs were a negative mirror image of the so-called true faith.

In their most disastrous form, the witch-hunts lasted until shortly after the end of the Thirty Years' War in 1648. Although there are records of sporadic witch trials and executions until 1750 in some places, 1650 marked the end of the worst and most remarkable aspects of the witch craze. During the whole period, between 200,000 (the most conservative estimate) and half a million people were executed (burned, drowned, beheaded, strangled, or hanged)⁴ on accusations of witchcraft (Currie 1968, p. 10; Kittredge [1929] 1972; Robbins 1959, pp. 16–17). “. . . Germany was almost entirely occupied in building bonfires. . . . Switzerland had to wipe out whole villages in order to keep them down. Travellers in Lorraine may see thousands and thousands of stakes . . .” (Trevor-Roper 1967, p. 152).

Evidence indicates that the majority of the witch craze's victims were women (Garrett 1977). In one specific area in southwest Germany, females constituted 85% of all victims (Midelfort 1972, pp. 179–80). Monter (personal communication, 1976) claims that a comparison of his research with that of Midelfort shows that nearly 90% of the victims were women. Lea (1957, p. 1079) reports that in Switzerland “. . . almost every woman . . . was considered . . . a witch. . . .” In “. . . (Weisensteig and Rottenberg) . . . we find overwhelming proportions of women (98–100 per cent) . . .” (Midelfort 1972, p. 179).

It has also been observed that the witch-hunts were conducted in their most intense form in those regions where the Catholic church was weakest (Lea 1957; Rose 1962) (Germany, Switzerland, France). In areas with a strong church (Spain, Poland, and eastern Europe) the witch craze was

⁴ Hsu (1960, p. 35) places the numerical limits between a minimum of 30,000 victims and a maximum of several millions.

negligible.⁵ Finally, we must realize that the description of the witch cult here is, necessarily, only partial and inexhaustive.

WITCHCRAFT: POSSIBLE EXPLANATIONS

Aside from the early "arm-chair anthropologists" (i.e., Frazer [1922] 1963; Lévy-Bruhl [1923] 1966), the anthropological empirical work on witchcraft—most of it carried out in so-called primitive societies—can be classified according to three major theoretical approaches. The first is a functional approach which views witchcraft as serving certain "useful" functions such as the alleviation of anxiety, integration, and the creation of cohesion (Evans-Pritchard 1937; Kluckhohn [1944] 1967; Malinowski 1955; Smelser 1962, pp. 95–100). The second is a structural approach which sees accusations of witchcraft as part of the relation network among different groups in a stratified society. A structural explanation could suggest that witchcraft accusations arise in times of rapid social change, because of social inequalities, various authority systems, or links with the social structure itself (e.g., Douglas 1966, 1967; Gluckman 1963; Marwick 1952; Nadel 1952; Turner 1957; Wilson 1951). A third approach (Lévi-Strauss 1963, chaps. 9 and 10; Turner 1969) analyzes the symbolic level in witchcraft rituals, focusing on the utilization, meaning, efficiency, and universality of various symbols used in magical practices.

Some scholars also deal with the phenomenon from a medical point of view. Forbes (1966), Harner (1973), and Harris (1974, pp. 178–92) argue that flying and other "hallucinations" experienced by witches were side effects of certain chemicals—highly hallucinogenic ingredients present in the ointment with which the witches smeared themselves. Psychological approaches focus either on the witches as presenting psychiatric symptoms (Caulfield 1943; Sarbin 1969; Schoeneman 1977; Zilboorg and Henry 1941; Zilboorg 1935) or on the emotional needs of the accusers (Anderson 1970; Rosen 1968, p. 15; Szasz 1970).

Another group of theoreticians tried to explain the witch-hunts as a variety of minority-group persecutions (Lewis 1971; Trevor-Roper 1967), in which witches were used as scapegoats for political, social, or economic purposes (Nelson 1971; Currie 1968; Schoeneman 1975). Typical is Harris's (1974, p. 205) explanation that "... the ... significance of the witch

⁵ By this time, the "church" was no longer a single unit, as evidenced by the religious turmoil among competing churches in Switzerland, France, and Germany. Nonetheless, in using the term "the church," I refer specifically to Catholicism as the religious social structure that, more or less, held Europe together over an extended period of time.

mania . . . was that it shifted responsibility for the crisis of late medieval society from both Church and State to imaginary demons. . . ."⁶

Recent research, however, reveals that while old, single women were persecuted at the beginning of the craze, they were replaced by younger and married women toward its middle and end. Except for these shifts in the choice of victims, no particular group of women was spared from persecution. This finding contrasts sharply with psychiatrically oriented approaches, according to which only bizarre or socially unacceptable women were persecuted as witches, and strongly supports the ideas presented in this paper. The European witch craze of the 14th to 17th centuries was a unique historical combination of accusations against people, especially women, of whom the overwhelming majority were probably completely innocent, and the creation of a theological system in which witchcraft became a phenomenon of central importance.

Therefore, most of the explanations of witchcraft mentioned above deal with functionally different phenomena from the European witch craze of the early modern era. While the theory of scapegoating fits the phenomenon under consideration, it does not explain its religious importance and intensity, its intellectual elaboration, the questions of timing and distribution, or the selection of women⁷ as its target. In the following pages an attempt will be made to identify the conditions which can explain these details.

TIMING: WHY DID THE WITCH-HUNTS BEGIN?

The witchcraft myth was created and crystallized largely by a number of Dominican friars. Until the 13th century, the Catholic church's official policy regarding witchcraft was summarized in the *Canon episcopi* (Lea 1957), which regarded belief in witchcraft as mere illusion.

The Inquisition was founded in the 13th century in order to combat the deliberate, continued, and public denial of the church's doctrine by baptized Christians, primarily by the Cathari but also, to a much lesser extent, by the Waldenses (Lea 1901, 1:3; Madaule 1967; Nelson 1971;

⁶ One group of writers gave credence to the very existence of the witch cult, claiming that the varying frequency of trials simply reflected the varying prevalence of witches. Many modern occult books and movies hold this position; among their predecessors we find Summers (1956), Murray (1921), Michelet (1965), and nearly all of the medieval demonologists (Robbins 1959, pp. 145-47).

⁷ Very few authors attempted to explain why women were the main victims of the witch-hunts, and only recently did a few bother to examine relevant statistics; see Garrett (1977); Matalene (1978); Midelfort (1972); Nelson (1971); and (though unsatisfactory) Ehrenreich and English (1972).

Wakefield and Evans 1969). Thus the Inquisition's primary objective was to single out and reconvert heretics.

In 1203 Saint Dominic first met the Albigensian heretics in Southern France; three years later he started a campaign to bring them back to the faith. In July 1216, Pope Innocent III, for whom he conducted his campaign, died, and the pope's successor, Honorius III, finally formally sanctioned Saint Dominic's new order—the Dominicans (December 22, 1216). The new order was established with the express hope that it would win back various groups of heretics to the church (i.e., the Albigensians, the Waldenses, who flourished in the early 12th century, and most important, the Cathari, who flourished in the 12th and 13th centuries in western Europe [Hinnebusch 1966; Mandonnet 1944]).

The pope's expectations of the Dominican Order were never realized, however; it failed to bring the lost sheep back to the fold, especially the Cathari. Between 1208 and 1213, Innocent III initiated a lengthy military crusade against heretics in Southern France in general and the Cathari in particular (Madaule 1967). The swift and bloody battle at Montségur in 1243 marked the end of the Cathari movement as a serious threat to the church. Most of the Cathari who remained were driven underground, and many of the French Cathari fled to Italy. Although the heresy lingered through the 14th century, most of it had faded in the 1270s; it finally disappeared in the 15th century (Puech 1974; Turberville 1964). The Waldenses received a similar treatment. By the end of the 13th century, persecution had virtually eliminated the sect, and by the end of the 15th century the survivors were segregated, for the most part, in the Italian and French valleys of the Alps (Cohn 1975).

Although the Cathari and Waldenses were probably the most visible and important heretics at the time, they were not the only ones (Lerner 1970; Loos 1974; Russell 1971; Runcimon 1955; Turberville 1964). For example, there were the Hussites who flourished from the end of the 13th century to about 1430, John Wyclif (1330–84) and his followers, and later, the Lollardy (extinguished in 1431), the Flagellants who flourished from about 1260 until the mid-15th century (their last persecutions and trials occurring in 1446–81) (Leff 1967), and the dancing manias (Rosen 1968).

However, by the 1250s practically no heretics were left to be pursued by the Inquisition (Lea 1901; Nelson 1971; Szasz 1970; Trevor-Roper 1967; Turberville 1964). The two major heretic factions—the Cathari and Waldenses—were in essence eliminated, while other groups were either too small or controlled with an iron hand. In order to justify the continuation of the Inquisition's machinery, the inquisitors began to search for new apostates. "The Inquisition . . . set about to introduce and develop the parallel heresy of witchcraft, thereby widening its scope. . . ." (Robbins

1959, pp. 207-8); also a campaign was initiated in Rome to extend its jurisdiction to the infidel Jews⁸ and the Moors of Spain.

While the inquisitors immediately began a campaign to extend their jurisdiction to the more "traditional" scapegoat, the Jews, they simultaneously demanded, from the 13th century on, that their authority be expanded to include witches they claimed to have found in the Pyrenees and the Alps.⁹ Their efforts were yielding results early in the 14th century when Pope John XXII issued his *Super illius specula*. When, however, between 1487 and 1489, the atrocious *Malleus maleficarum* was printed, with the blessings of Pope Innocent's witch bull, *Summis desiderantes*, the "art of witchcraft" had reached its peak, and the inquisitors' desire to control witchcraft was almost totally realized.

Although important and authoritative, the *Malleus maleficarum* was by no means the first, or the only, book about witches published in the 15th century. Approximately 15 different books were published on the subject after 1435 (the date of Nider's *Formicarius*). Of these, Jacquier's *Flagellum haereticorum fascinariorum* (1458) proved to be somewhat of a turning point.

Jacquier defined witchcraft—for the first time—as a new, evil form of heresy (Lea 1901, 3:497; Robbins 1959), claiming that witches were qualitatively different from the rest of humanity. It should be recalled that until this time the Catholic church's official position on witchcraft was based on the *Canon episcopi*,¹⁰ which pointed out that beliefs in witchcraft were mere fantasies and that he who believed in witchcraft had lost his faith. Jacquier's problem was how to cope with this statement in terms of his own beliefs in witchcraft. His solution was swift and clear: he stated that the existence of the "witch sect" indicated the existence of witches qualitatively different from the ones to which the *Canon episcopi* alluded. He claimed that witches were organized and used to fly to the atrocious Sabbath ceremonies, where they indulged in sexual orgies with the devil and feasted on roasted unbaptized infants. This paved the way for the new conception of the witch.

⁸ The Inquisition in the Iberian peninsula kept persecuting Jews until the 17th century (Eliav 1971; Roth 1971; Wolff 1971), which explains in part the absence of European-style witch-hunting there.

⁹ Already in 1257 the Inquisition pressed the papacy for inquisitorial power over witchcraft, but the papacy resisted (Trevor-Roper 1967, p. 108).

¹⁰ The origin of that document is not entirely clear. Kors and Peters (1972, pp. 28-31) date it to 1140 and find mention of it in *The Concordance of Discordant* by Gratian. However, they no doubt used the English translation of H. C. Lea (1957, 1:178-80). Lea himself (1957, p. 494) attributed the *Canon episcopi* to an obscure meeting, the Council of Anquira, held perhaps in the 4th century (although there does not seem to be any record of such a council). Notwithstanding the obscurity surrounding its origins, the *Canon episcopi* was definitely adopted by later canonists as official policy concerning the matter of witchcraft.

Some 30 years later, the *Malleus maleficarum* appeared; it was to become the most influential and widely used handbook on witchcraft. The book is divided into three parts. The first section attempts to prove by argumentation (rather than factual demonstration) the existence of witches and devils. It states that he who does not believe in the existence of witches is himself a victim of witchcraft practices (Szasz 1970)—a clear departure from the policy of the *Canon episcopi*. The second section tells how to identify a witch—what signs, techniques, and tests to use. This is important because previously there had been no readily available, easy definition of a witch. The third section of the book describes the legalities of investigating and sentencing a witch.

The *Malleus* explicitly connected witchcraft with womanhood, explaining that "... witchcraft is chiefly found in women ..." because they were more credulous and had poor memories and because "... witchcraft comes from carnal lust, which is in women insatiable ..." (Sprenger and Kramer [1487-89] 1968, pp. 41-48). The book's recommended way to destroy the devil was to burn his host (the witch) "... using green wood for the slow burning of the grossly impenitent ..." (Sprenger and Kramer [1487-89] 1968, p. 220). The authors specifically encouraged the use of torture to elicit confessions.

The importance of the *Malleus* cannot be overestimated. Its enormous influence was practically guaranteed, owing not only to its authoritative appearance but also to its extremely wide distribution. It was one of the first books to be printed on the recently invented printing press (Trevor-Roper 1967, p. 101) and appeared in no fewer than 20 editions (Zilboorg and Henry 1941). It became the textbook of the Inquisition, and, with the insertion of the *Summis desiderantes* as its preface, the last impediment to the inquisitorial witch-hunt was removed.¹¹ The moral backing had been provided for a horrible, endless march of suffering, torture, and human disgrace inflicted on thousands of women.

This account clearly links the beginnings of witch-hunts to the vested interest of the Dominicans and the Inquisition. They had a professional interest in the discovery of problems and of populations on which to exercise their specialized theological expertise in heresies and their investigative skills. The fact that they showed much less interest in witches in Spain and Portugal than in other countries of western Europe is consistent with this hypothesis of professional interest. In the Iberian peninsula the persecution of Jews and Moors provided them with plenty of intellectual challenge and employment.

¹¹ Lea (1957, 1:338-45) and Zilboorg and Henry (1941, p. 151) provide vivid and accurate descriptions of the various techniques of deception used by Sprenger and Kramer in order to authenticate their book.

The End of the Medieval Order

The professional interest of the inquisitors explains why they began to take an interest in witches as early as the 13th century. But the transformation of this interest into an elaborate demonological theology took place only in the 15th century, and only then did the general public begin to share increasingly the interest of professional inquisitors in witches. To explain these developments requires a broader perspective on the social, institutional, intellectual, and emotional changes which culminated between the 15th and 17th centuries.

During the 13th to 17th centuries in general, and the 14th to 17th centuries in particular, the medieval social order underwent a series of significant changes which completely altered the dominant European outlook.

According to Pirenne (1937), the growth of cities and of an industrial form of production started in the Low Countries and in England in the 12th century, and from there reached down the Rhine in the 13th century. Among the changes that took place in the economic expansion of the 13th century was the development of numerous cities in Flanders (reaching a peak in the 14th century [Nicholas 1976]) and England, a significant increase in population size, perfection of the monetary system, and the mapping of new lands (Pirenne 1937, pp. 189-90; Thrupp 1972; Le Goff 1972; Bernard 1972). The expansion of commerce was not limited to central Europe, as ore mining began in Poland (Molenda 1976) and Mediterranean trade flourished at the same time (Ashtor 1976).¹²

This economic development brought with it increased trade, expanded urban industry, standardization, exports, division of labor, and specialization (Bernard 1972; Thrupp 1972; Le Goff 1972). By the end of the 13th century and the beginning of the 14th, "... the development of industry and commerce completely transformed the appearance and indeed the very existence of society ... continental Europe was covered with towns from which the activity of the new middle-class radiated in all directions ... the circulation of money was perfected ... new forms of credit came into use ..." (Pirenne 1937, pp. 189-90; see also Le Goff 1972; Russell 1972). All this was only the beginning of a process the peak of which was reached in the 14th to 16th centuries. These centuries proved a turning point not only in commerce but also in geographical discoveries and their utilization (Postan and Rich 1952; Postan, Rich, and Miller 1963). "The exploration and exploitation of non-European areas by Europeans during the

¹² The economic development originally described by Pirenne has been corroborated by many other scholars. Cipolla (1976; 1978, p. 32) called it a "Commercial Revolution" and "a sort of Industrial Revolution." Other sources corroborate the fact that 11th- to 13th-century Europe experienced intensive industrial, commercial, and monetary developments (Bernard 1972; Carus-Wilson 1941; Earle 1969; Lane 1933; Lopez 1976; Thrupp 1972).

15th and 16th centuries form one of the greatest phenomena of the Renaissance . . ." (Penrose 1962, p. vii), no doubt forcing ". . . a re-evaluation of the idea of Europe as a model Christian society . . ." (Rattansi 1972, p. 7).

These extreme and relatively rapid changes made deep inroads in the hierarchic structure of feudal society sanctioned and legitimized by the Catholic church. In the medieval tradition, the moral boundaries of society were clearly defined. There was Christendom ruled spiritually by Rome and structured in a uniformly conceived hierarchic feudal order, firmly embedded in a finite cosmic order ruled by God. This order was threatened by the heretic Jews and Moslems, but their heresies were already related to the Christian tradition, and the relationship to them was clearly defined: they had to be converted and saved or, if recalcitrant, fought and suppressed.

This order became increasingly threatened by the rise of urban society that did not fit into the feudal hierarchy, by the growth of contact with non-Christian people that did not fit the conversion-conflict model, and by the resultant autonomy of economic and political transactions from theological guidance.

Stress and confusion created by these circumstances were further aggravated by external catastrophes, especially the devastating epidemics of plague and cholera which decimated the population of Europe and lasted throughout the 14th century. Furthermore, even the physical climate of Europe underwent severe changes in those fateful centuries, as evidenced by the onset of the Little Ice Age (Russell 1972, pp. 51-52), and the appearance of the great comet of 1528, which caused great fear and anxiety.

Stress and confusion, however, were only one aspect of these developments (Holmes 1975). There was confusion about the moral boundaries of society and the cognitive map of the world; frequently there was fear of impending doom. But there was also an opening up of new possibilities and a rise of standards of living in the wake of the great catastrophes of the 14th century. Those who survived the epidemics inherited the wealth of the deceased, and even those who had to maintain themselves by their work could obtain far better wages than before because of the shortage of manpower.

Thus the 15th century was a time of great enterprise, bold thought, innovation, as well as one of deep confusion and anomie (Durkheim 1951), a feeling that society had lost its norms and boundaries and that the uncontrollable forces of change were destroying all order and moral tradition. These developments allowed many contemporary thinkers to overstep the boundaries of reality and enter the realm of magic, fancy, and make-believe. Thus during the period between the 15th and 17th centuries there

was frequently no clear demarcation between rational science and magic.

The inquisitors were forming their demonological theories in the early years of the scientific revolution (Ben-David 1971; Debus 1970; Rattansi 1972), when pseudoscience was rarely distinguished from other forms of science (Thorndike 1941; White 1913). There was great preoccupation with so-called secret (or esoteric) knowledge, namely, the Hermetic movement (Seligmann 1948), which "... focused attention on ... extraordinary and marvelous virtues. ... The aim was to grasp the hidden powers of nature and the mysterious forces. ..." (Rattansi 1972, p. 5). This explains why "... the growth of demonology and of the witch-hunt mania paralleled that of the scientific revolution. ..." (Kirsch 1978, p. 152) as well as a rise of interest in social utopias and "ideal societies" in the early decades of the 15th century (Cohn 1961; Graus 1967), which was another reaction to the dissolution of the cognitive and moral boundaries of the medieval world. The anomie resulting from the uncontrolled changes called forth positive as well as negative reactions. The expansion of horizons and the instability of social conditions, the Reformation, the beginning of the scientific revolution, Renaissance art, and humanism took advantage of the disappearance of traditional norms and boundaries for the creation of greater cultural diversity and freedom, giving rise to a new, infinitely more differentiated culture than that of the Middle Ages. The witch craze was a negative reaction in the sense that its purpose was to counteract and prevent change and to reestablish traditional religious authority.

Talcott Parsons's views on the development of modern European society are worth considering at this point. In his analysis, he contends that the traditional feudal system began to differentiate during the 11th century, starting a process which led—by the 17th century—to an increasing autonomy of the religious, governmental, and economic institutions. The new social order, based on relatively autonomous institutions, replaced a previous rigid, religiously defined, and more or less unified social system (Parsons 1966, 1971). The social change affected the very "center" or the "collective conscience" of society (Durkheim 1964; Shils 1975), or, to use Parsons's own term, the definition of the "societal community." In simpler terms this means that there was a newly felt need for the definition of the moral boundaries of society. By persecuting witches, this society, led by the church, attempted to redefine its moral boundaries. This was one of the numerous instances in which deviance served the social functions of emphasizing and creating moral boundaries and enhancing solidarity (Cohen 1966; Durkheim 1950; Erikson 1964, 1966; Lauderdale 1976). In fact, the witch craze was fictitious deviance, created for those purposes.

Until the Renaissance, the Catholic church was at its peak of power. All problems were treated as theological or theosophical, and the moral boundaries of society were well defined with no serious threat to them.

This is the reason that during the so-called dark Middle Ages we have hardly any record of witch crazes.

Once the results of the differentiation process became visible in the 15th century, and a sharp decline in the church's authority was noticeable, "... the church began to need an opponent whom it could divinely hate ..." (Williams 1959, p. 37), so that a redefinition of moral boundaries could take place. This helps us understand why only the most rapidly developing countries, where the Catholic church was weakest, experienced a virulent witch craze (i.e., Germany, France, Switzerland). Where the Catholic church was strong (Spain, Italy, Portugal) hardly any witch craze occurred, even if these were rapidly developing societies, such as the Italian city-states. Although this was not the first time that the Catholic church was threatened, this development culminating in the Reformation was definitely the first time that the church had to cope with a large-scale threat to its very existence and legitimacy (Elton 1963), as "... the Reformation shattered the unity of Christendom, and religious conflicts ... destroyed the illusion of the perfect Christian societies ..." (Rattansi 1972, p. 7).

For this reason, Protestants persecuted witches with almost the same zeal as the Catholics, despite many objective differences between them. Protestantism might have been a result of the differentiation process, but this is not to say that Protestants were able either to master or to steer the process itself. Protestants and Catholics alike felt threatened both by the process and by each other.

Dominican theory portrayed witchcraft and witches as the negative mirror image of the true faith. This made it possible to attribute all the undesirable phenomena associated with the anomie of the age to the conspiracy of Satan and the witches against Christianity. By associating everything negative with witchcraft, the ideal components of the true faith were positively highlighted. In this sense, the witch craze could be called a "collective search for identity" (Klapp 1969) and the authors of the atrocious *Malleus maleficarum* can be seen as "moral entrepreneurs" (Becker 1963, pp. 147-63), taking part in a "moral crusade" (Gusfield 1967), striving to restore the integrity of the old religious-moral community. Witches were the only deviants who could be construed as attacking the very core of the social system, through antireligion.

This explains why a number of theologians and intellectuals found in the demonology of witches a cognitively satisfactory diagnosis of the moral ills of their time. It still has to be explained how this abstruse theory became so readily accepted by the masses.

As a result of the severe socioeconomic stress, the entire feudal social order crumbled and "immense sadness and a feeling of doom pervaded the land" (Anderson 1970, p. 1733), intensified and aggravated by the severe

climatological changes, demographical revolutions, and the disruption of family and communal life, all of which were perceived as signs of impending doom (Connor 1975). Furthermore, "... the individual was confronted with an enormously wider range of competing beliefs in almost every area of social and intellectual concern, while conformity-inducing pressures of a mainly ecclesiastical sort were weakened or discredited . . ." (Rattansi 1972, pp. 7-8). The existential crisis of individuals—expressed in terms of anomie, alienation, strangeness, powerlessness, and anxiety—created a fertile soil in which the Dominican solution could flourish.

What could better explain the strain felt by the individual than the idea that he was part of a cataclysmic, cosmic struggle between the "sons of light" and the "sons of darkness"? His personal acceptance of this particular explanation was further guaranteed by the fact that he could assist the "sons of light" in helping to trap the "sons of darkness"—the despised witches—and thus play a real role in ending the cosmic struggle in a way that would bring salvation nearer. Thus the differentiation process threatened not only the macroinstitutional level but also the microlevel—each person's individual cognitive map. In such a case, a redefinition of moral boundaries and a restructuring of cognitive maps would be more than welcome: for this reason, the witch craze won extensive popular support.

WITCHCRAFT AS AN IDEOLOGY¹³

What made the demonological theory so attractive to the masses was that it had all the characteristics of what could be considered an effective ideology. The concept of ideology referred to is Geertz's (1964). Although he limits his discussion to situations in which the need for cognitive and moral reorientation is the result of the emergence of "autonomous polity," namely, the differentiation of the political from the religious sphere, widespread need for such reorientation is caused by every process of significant institutional differentiation. Such processes create a disturbing discrepancy between what is believed and reality. The function of ideology is to provide authoritative concepts capable of rendering the situation meaningful and "suasive images" by which the meaning can be "sensibly grasped" and which can arouse emotions and direct mass action toward objectives which promise to resolve existing strain.

The existence of widespread strain due to the inadequacy of traditional concepts, especially in the religious-moral sphere, has been documented above. However, it is possible to show that much of this strain became particularly focused on women, which explains why witches—usually female ones—could become such effective symbols in a new ideology.

¹³ I am grateful to Joseph Ben-David for suggesting the connection between ideology and the witch-craze phenomenon.

How women became such a symbol can be explained through three events: structural and functional changes in the family, changes in the status and role of women, and demographic changes.

Ariès (1962) points out that the medieval family was a property-holding unit, and "... the home of the early Middle Ages was the heart of the industrial life of the community ..." (Goodsell 1915, p. 207). In this home, the woman possessed a central role, both as a housekeeper and mother and as a breadwinner. For example, "... in the 7th century the textile industry was wholly carried on by wives and daughters of the family. ... Such was the case prior to the 12th century when weaving ... [increasingly became] ... a skilled craft in the hands of the men ..." (Goodsell 1915, p. 208). Goodsell's book leaves the reader with a firm impression of the medieval wife's hard life.

According to Jarrett (1962), the main functions of a married woman were (1) to provide male heirs for the family's property and (2) to make her husband richer by the treasures she was supposed to bring as her dowry (also in Bullough 1974; Goodsell 1915) and as a producing unit. Jarrett's main theme is the subordinate status of medieval women.

The social position of medieval women is a problematic issue, and researchers have only recently begun to explore it seriously (e.g., Herlihy 1971; Morewedge 1975; Power 1975; Stuard 1976). Power (1926, 1975) states that the social position of medieval women was far from clear in that it was the subject of an ongoing dispute between the church and the aristocracy. She claims, however, that everyone seemed to accept the subordination of women, and that the prevailing attitude toward women was one of possession. There were also those who regarded women as dangerous and seductive, or even as superior beings (as in the Virgin Mary cult) (Warner 1976); this view seems inconsistent with subordination of women. Part of Power's conceptual confusion stems, no doubt, from her lack of attention to dates, but, nonetheless, she provides us with a clear example of the subordinate role of medieval women (the inconsistencies will be dealt with later).¹⁴

Toward the end of the 13th and beginning of the 14th century, a time ripe with economic upheavals and changes, many families moved from the rural areas to towns, changing their economic outlook and shifting from producing and exchanging goods to a purely cash economy. This shift had a number of consequences: (1) A family of this type could hardly afford to support ill, unemployed, or unproductive members. (2) The family

¹⁴ Several recent studies (Bainton 1971, 1973; Chojnacki 1974; Coleman 1971; Gies and Gies 1978; Herlihy 1971; O'Faolain and Martines 1973; Stuard 1976) generally tend to corroborate the conception of the subjugation of medieval women but also point to specific instances, periods, and situations in which women successfully raised their status.

structure's main function changed from a property-holding, working unit to a consuming unit. (3) As a result of the great number of peasants coming to town in the 13th century, the worker's real wages remained very low, and any fluctuation in business caused severe survival problems. This situation understandably produced considerable insecurity among the new city dwellers (Cohn 1961; Helleiner 1967). (4) Consequently, male employees in large-scale enterprises (textiles, flour mills, mining) subsisted too close to the starvation level and could not afford marriage. Moreover, "... marriage of the artisan depended on admission to masterhood, and this in turn depended on conditions which favored the masters of the guild. Guild regulations prohibited admission of married apprentices . . ." (McDonnell 1954, p. 84; see also Wrigley 1969).¹⁵

These factors, when combined, created strong pressures on women to enter the job market, either to support their families if they had any, or to support themselves if they were alone. The fate of the unmarried girl was more or less sealed (Goodsell 1915, p. 210). Some were sent to convents (McDonnell 1954), others could stay with their families and help with work. In the cities, however, women without mates or families to support them and with no chance of entering a convent usually worked in spinning and weaving. Some also resorted to prostitution (McDonnell 1954). Sources on the history of prostitution (Bullough and Bullough 1964; Henriques 1963; Sanger 1937; Scott 1936) attest to the sharp increase in the number of prostitutes in growing urban industrial centers of the 14th and 15th centuries. La Croix (1926, vol. 2) points out that cities along the Rhine and in Alsace-Lorraine (where new industries were developing) had instituted numerous laws against prostitution by the end of the 14th century. (These places, incidentally, were also characterized by a high degree of witch-hunting.) It hardly seems coincidental that Sprenger—one of the authors of the *Malleus*—came from Cologne, the principal industrial and commercial city on the Rhine (Nelson 1971, p. 25).

During this period numerous rich families attempted to establish secular convents to which they could send their unwed daughters, but these convents deteriorated rapidly and were later turned into hospitals and poorhouses (McDonnell 1954, pp. 82, 84; Nelson 1971, p. 25).

The pressures on women to enter the job market were thus very strong, and women responded by entering various newly industrialized spheres. Consequently, during the 13th and 14th centuries, women's differing roles as part of the traditional family structure and as unmarried workers became very problematic.

¹⁵ This rule, essentially, was an attempt by those in power to keep it. As Marx pointed out, this development also contributed to the rise of a permanent proletariat.

An initial reaction to this dilemma was to glorify the old, traditional role of the woman. Women thus became objects of worship, appreciation, adoration, and admiration (Nelson 1971, p. 24). Lea (1901, 3:597-610) and Warner (1976), for example, report on the growing support generated by the Virgin Mary cult, in which the Virgin was worshipped as the ideal woman. However, this attitude did not last very long, and the image of the woman soon changed.

The 14th Century: New Patterns Emerge

During the 14th century, Europe experienced severe demographic changes which bear directly on the concentration on women as victims of witch-hunts. A major disaster altered the situation, as well as the image, of women: the devastating Black Death (1347-50). The mortality rate was particularly high in cities owing to greater density of population and the absence of hygienic conditions (Helleiner 1967). Lea (1901, vol. 2) reports certain places where, out of each 1,000 people, barely 100 survived.

This constituted a turning point in the demography of Europe (Russell 1972; Borrie 1970; Wrigley 1969). "Central and northern Europe . . . saw a three-fold growth in the pre-plague period with its most rapid advance from about 1150-1200 to 1300. . . . In this period, for the first time, cities larger than 20,000 appeared . . ." (Russell 1972, p. 40). Paris and London reached populations of 100,000 and 50,000, respectively (Russell 1972, pp. 34-35).¹⁶ The effect of the plague on the population was devastating. It can be assumed with a fair degree of certainty that 30%-50% of Europe's population was annihilated by this disaster (Bridbury 1973; Cipolla 1974; Langer 1964; Lea 1901, vol. 2; Russell 1972; Usher 1956; Ziegler 1971). Postan (1950), for example, reports that the decrease in population was so sharp, with no corresponding increases during following centuries, that almost 50%-60% of the land was deserted in Denmark, Sweden, Norway, and Germany.¹⁷

After the major plagues had passed, peasant and wage-laborer survivors found themselves in a highly advantageous position: as a result of the shortage in manpower (Spengler 1968, p. 433), their real income was tremendously increased, food improved, and job security magnified. In addition, many survivors had inherited large amounts of wealth from their

¹⁶ Russell also indicates that until the 14th century, there had been more males than females in the population. (No indication is given as to exactly when a better female/male ratio was achieved.)

¹⁷ In England the situation was different. Although the plague hit England quite hard, the country's large population surplus counterbalanced some of its effects and probably affected the emerging patterns of witch-hunting there, too (see, e.g., Bridbury 1973, 1977; Postan 1950; Usher 1956).

deceased relatives (Langer 1964). Under such favorable conditions, one might expect an increase in the population, but this did not occur (Nelson 1971; Noonan 1968; Spengler 1968; Deevey 1960; Helleiner 1957) before the 16th to 17th centuries (Helleiner 1957; Langer 1964; Wrigley 1969). This phenomenon can be explained in part by the sporadic, unpredictable reappearance of disease, as well as the continuation of the Hundred Years' War. But the essential explanation lies elsewhere. The fact that the birthrate decreased and the population did not increase in the second half of the 14th century can be explained by the widespread use of contraception and by the rise in infanticide (Helleiner 1967, p. 71). Why these practices were used can be easily explained.

Because part of the population was—quite suddenly—exposed to a high standard of living due to an increase in real income (wages), these people did not want to undermine their new prosperity by raising large families. Furthermore, the economic, monetary, commercial, and urban revolutions which accompanied the Renaissance and Reformation probably also gave a powerful stimulus to the rise of basic forms of individualism and egoism. Those who married took care to limit their number of offspring, while those who did not marry made efforts to prevent pregnancy (Spengler 1968, pp. 436–37, 440). The church at that time complained bitterly of the widespread use of *coitus interruptus* by married and single persons alike as a means of preventing pregnancy (Himes 1936; Noonan 1965; Wrigley 1969, p. 124). Although historical research on infanticide is still itself in its infancy and cannot yet provide us with reliable numbers concerning the scope of the phenomenon in the 12th to 15th centuries, a growing number of scholars have suggested that the rate and scope of infanticide increased sharply and significantly during the period in question (Coleman 1976; Davis and Blake 1956; De Mause 1974, pp. 1–183; Goodsell 1915, chaps. 6 and 7; Helleiner 1967; Helmholtz 1975; Himes 1936; Langer 1974a, 1974b; Noonan 1965; Radbill 1974; Spengler 1968; Trexler 1973; Wrigley 1969, pp. 125–26).¹⁸

Coleman (1976, p. 57) notes that “. . . many children were left abandoned at a church's door; and they were accepted in order to prevent their death at the hands of their parents . . .” and that “. . . the purpose of . . . infanticide was to regulate children, not eliminate them . . .” (p. 69).

It is quite clear that the 15th and 16th centuries brought one of the most severe demographic changes Europe had ever experienced. First, the

¹⁸ This problem began prior to the 14th century. Already at the end of the 12th century, for example, Innocent III established a hospital in Rome “. . . because so many women were throwing their children into the Tiber . . .” and earlier than that “. . . there were as many infanticides as there were infants born out of wedlock . . .” (Trexler 1973, p. 99).

unique European marriage patterns, characterized by (1) a late age of marriage and (2) a high proportion of people never marrying at all, took shape roughly from about the 15th and 16th centuries. The basis of these patterns was the insistence of single persons on achieving a certain standard of living as a prerequisite to marriage (Hajnal 1965; Noonan 1968; Russell 1972; Spengler 1968; Wrigley 1969). Second, the marriage age of single males rose to 25 and more (Litchfield 1966) and of females to 23 and even 27 (Midelfort 1972, pp. 184-86). These developments created high proportions of unmarrieds—a significant process in a society which attaches a stigma to being single. In turn, these processes motivated the development of convents (Litchfield 1966), the rise of Beguines in the 15th and 16th centuries, and the advent of the spinster in Protestant countries. Wrigley (1959, p. 90) succinctly summarized these developments by stating that “. . . between two-fifths and three-fifths of the women of childbearing age 15-44 were unmarried. . . .”¹⁹

These figures indicate a definite shift to late marriage which was the focus for the crystallization of the nuclear family, on one hand, and marriage by choice, on the other (Noonan 1968, p. 468). The large increase in the number of unmarried women produced a serious problem and may be the background to the fact that a significant number of “witches” were either widows or spinsters (at least when the persecutions started). Later on, however, married women and young girls were persecuted as well (Midelfort 1972).

It is evident from all this that, beginning in the 12th century and throughout the entire period dealt with here (to the mid-17th century), the social role of women was in constant flux. Urban industrial life compelled them to step outside the traditional role of the housewife (Nelson 1971). Whereas two centuries earlier they could not get married because men could not afford marriage, in the 15th century they were unable to marry owing to men's reluctance to marry. The change in woman's traditional role hinged on the fact that she entered a competitive market characterized by lack of manpower. The resulting entrance of women into this competitive job market produced a virulent misogynic ideology (Bainton 1971, pp. 9-14; Kelso 1956; Midelfort 1972, p. 183).

There were also deep changes in women's role as mothers. There was the widespread use of contraception and infanticide, which the church strongly and fiercely denounced as most evil. Trexler (1973, p. 98) notes that “. . . child-killing has been regarded almost exclusively as a female crime, the result of woman's inherent tendency to lechery, passion, and lack of responsibility. . . .” He adds (p. 103) that “. . . infanticide was

¹⁹ These changes took place progressively over Europe. In many areas they were present as early as the 15th and 16th centuries; in others, they may have been present already in the 14th century (Wrigley 1969; Herlihy 1965).

far and away the most common social crime imputed to . . . witches . . . by the demonologists . . ." (see also Lea 1957, vol. 1; Murray 1918; Spren-ger and Kramer [1487-89] 1968). Infanticide was not only a result of the fact that many children were born out of wedlock. At that time, it was felt that breast-feeding was the best way of helping a healthy infant grow. Alas, many rich women either could not breast-feed their offspring or did not want to. Consequently, wet nurses were sought. There are indications that many wet nurses were poor women hired after their infants either died naturally, or, more often, were killed. Trexler (1973) suggests that it is quite possible that, in many cases, becoming a wet nurse was a planned course of action, as a safe, comfortable source of income.

No wonder, then, that midwives were among the chief suspects of witchcraft (Forbes 1966), since the Dominicans suspected—and probably right-ly—that midwives were experts in birth control and no doubt helped and cooperated in infanticide. This explains why, in numerical terms, women were the principal victims of the witch-hunts. The craze reflected changes in woman's traditional role and in the structure of the family.

Among the large numbers of unmarried men and women, there was apparently much sexual license, including religiously sinful contraception, and even such capital crime as infanticide. Under such circumstances the relationship between the sexes must have been frequently one of mutual exploitation and fraught with deep feelings of guilt and resentment. Because of the powerlessness of women under secular and religious law, and their inferior status, it was convenient to project on them all the resentment and guilt. The ideology of the witch-hunt made use of these emotions—it allowed men who indulged in sex that proved unhealthy for them to accuse women of taking away their generative powers. Those who were party to contraception through *coitus interruptus* could project their guilt feelings on women for stealing their seed. The fantasies about the unlimited sexual powers and depravity of women may have been a reflection of the fear engendered by the large number of unmarried women not subject to the authority of fathers or husbands, as, according to prevailing views, they ought to have been.

These feelings, reflected in the images of witch ideology, must have been very widespread among men, since presumably a large fraction of them took advantage of the prevailing sexual license. Among married women who probably did not, or could not, indulge in illicit sex, there must have been strong feelings against "bad women" who might "bewitch" their husbands and sons or who had actually done so. Therefore, the female witch, using sex for corrupting the world on behalf of Satan, was a "suasive image" of great power, in an ideology the aim of which was to cleanse the world from all the effects of social change and anomie and to restore the moral boundaries of medieval society.

TIMING: TERMINATION OF THE WITCH CRAZE

In their most devastating form, the witch-hunts lasted until after the Thirty Years' War (1648) ended in the so-called Peace of Westphalia. The invasion of foreign armies from the north halted the persecution of witches in many cases (Nelson 1971; Robbins 1959). By the 17th century the ideological basis behind the witch-hunts was clouded by growing doubts as to its legitimacy, and the various "technologies" and tortures used to hunt and isolate a witch were meeting with severe criticism. Eventually, power was taken away from the courts, the inquisitorial machinery was dismantled, and persecution of witches came to an end. This fact is consistent with the idea that once the differentiation process was well on its way and people began adjusting to the new situation, the persecutions ended. The coincidence of the termination of the craze with the termination of the Thirty Years' War in the Peace of Westphalia is not just a chance occurrence. This peace, no doubt, gave official recognition and legitimacy to religious pluralism and symbolically ended the European struggle to redefine the social system morally. In Parsons's (1971, p. 52) terms, "... religious pluralization was part of a process of differentiation between the cultural and societal systems that reduced the rigidity and diffuseness of their interpenetration. . . ." The internal stresses, insecurity, and instability experienced by persons living in the stricken areas during the Thirty Years' War, as well as the exhausting, debilitating struggle and the invasions of various military groups and armies, provided fuel to the burning furnace of the final phase of the witch craze. But once stability was achieved and religious pluralism accepted, the witch-hunts weakened, finally disappearing altogether. It is evident, thus, that by the 17th century there was a demarcation among science, magic, and religion. There were also a recognition of autonomy of government and economy in England, and a settlement of relationships elsewhere in a way which recognized supremacy of the political sphere. A new social order had visibly and triumphantly crystallized with trends and expectations stabilized, especially regarding the family, cognitive maps, initial separation of church and state, and the rise of science, all of which were now coming into their own. Furthermore, the new European pattern of marriage and spinsterhood was institutionalized in the 17th century, and the earlier turmoil surrounding the formation of these new life-styles subsided.

The ideas presented in this paper raise a question regarding the usefulness of ideology in general and the witch-hunts in particular. Were the witch persecutions successful in restoring the religious moral boundaries of medieval society? Evidently not. The medieval society was not reconstructed or restored, and probably could not have been restored. The effort of doing so was futile and the sacrifice of innumerable human lives could

not be justified even in any instrumental terms. Whether participation in witch-hunts helped people psychologically to survive the period of uncertainty and transition is a different question. Even if it did, it did so at the expense of sacrificing the lives of many for the passing psychological gratification of some, and it certainly did not make the persecutors into better Christians, which was the avowed purpose of the witchcraft ideology.

Furthermore, from this discussion and from Erikson's (1966) discussion of the Salem witch-hunts, it is evident that persecutions did not prevent change and that they failed in fulfilling their primary function, the redefinition of moral boundaries. Generally speaking, it appears, then, that when a community so vehemently and desperately tries to restore its moral boundaries, sociologists can expect that the attempt is doomed to fail. It is possible that the very attempt at restoration is in itself a symptom that a major change is taking place and that it is impossible to go back, so to speak, and redefine moral boundaries. In this sense, persecutions can be interpreted as a symbol of incapacity, of a system's failure, as "death throes," if you wish, and they might be viable proof that the previous equilibrium cannot be recaptured. Both cases—Salem²⁰ and Europe—seem to be sound proof of this idea.

CONCLUDING DISCUSSION

This paper has attempted to interpret the continental European witch craze as a result of the convergence of several conditions.

1. Timing of the witch craze and related questions: why did the witch craze start when it did, why did it end when it did, why was it accepted in such a widespread popular fashion, why was it distributed as it was?

The witch craze actually began in the 14th century when the inquisitors and the Dominicans had either to find new goals for themselves or to remain without a purposeful action and slowly disintegrate. However, that in itself was not enough. During the 15th and 16th centuries Europe experienced the painful birth pangs associated with the emergence of a new social order and the crumbling of an older one. I have referred to this as a differentiation process. The state of powerlessness and anomie experienced by contemporary individuals was further aggravated by severe climatological and demographic changes which, together with geographical discoveries, created a feeling of impending doom, thus paving the way for the widespread popularity of the craze. The dissolution of the medieval cognitive map of the world also gave rise to utopian expecta-

²⁰ In 1692 a witch-hunt craze broke out at Salem, Massachusetts, and lasted for about one year. Only a few dozen people were involved, and even fewer were executed. The history of that episode has been richly documented and analyzed; see, e.g., Erikson (1966); Robbins (1959, pp. 429-48).

tions, magical beliefs, and bold scientific explorations. These conditions created the need for a redefinition of moral boundaries as an attempt to restore the previous social order.

2. Content: why a *witch* craze and not something else? How do we explain the emergence of an antireligious ideology, focusing on the witches? The answer to this question lies precisely in the antinomian character of the ideology. By emphasizing the negative a finger was pointed implicitly at what should have been. Only demonology and witches could serve this purpose since no other heretical group—imaginary or real—threatened the basis of legitimacy of Christianity. The elaboration of witchcraft theories into a complex religious ideology in the form of demonology (and the elevation of witch-hunts into a sacred activity pursued with religious fervor) was a direct result of the need for such a theoretical construct as a reaction to the late medieval anomie. I also examined briefly the efficiency and success of the witch-craze ideology, comparing it with Erikson's (1966) analysis of the 1692 witch craze in New England's Salem, noting that in both cases persecutions did not fulfill their primary function and did not prevent or alter the relevant social changes.

3. Target: why were women the major victims of the craze? The analysis above indicates that changes in the economy, demography, and the structure of the family, especially changes in the role of women, explain the nature of the target. It is evident that the growth of the proportion of unmarried women, prostitution, infanticide, and contraception were a salient complex of problems, likely to arouse strong feelings. These conditions explain the suitability of a female symbol, such as the witch, to become an effective and central element in a demonological ideology. Furthermore, while the severe, even catastrophic, changes provided the intellectual cognitive background for the craze, its emotional zeal was made possible by focusing the tension, powerlessness, and anomie on a relatively safe, weak target—women. Women had an inferior status to begin with, and their lack of power and organization (Lewis 1971) rendered them ready targets for widespread persecutions.

The witch craze ended when the conditions for its inception were no longer in existence. The spatial distribution of the witch-hunts (and their termination) were direct results of the presence or absence of all or some of the conditions described above that prevailed in different parts of Europe during the period in question. The witch craze occurred in those countries and areas where the crisis was most deeply felt and the church was weak. Where the church was strong or progress was not marked (or both), hardly any witch craze occurred. The disappearance of these conditions everywhere in the 17th century inevitably meant the end of the craze.

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Determinants of Political Orientation: Class and Organization in the Parisian Insurrection of June 1848¹

Mark Traugott

University of California, Santa Cruz

The Parisian insurrection of June 1848 has been a major point of departure for theories which view class origin as a determinant of support for or resistance to revolutionary action. This article assembles data to show that opposing sides in the June Days were virtually identical with respect to class origin as measured by self-reported occupation. A comparison of the two groups most central to the insurrection and repression—the Parisian National Workshops and the Mobile Guard, respectively—suggests the alternative conclusion that variations in political behavior resulted from a combination of members' positions within the artisanal hierarchy and collective organizational experience between February and June.

In late February 1848, the constitutional monarchy of Louis Philippe fell in a brief and relatively bloodless "revolution." The assault on the Orleanist regime encountered only token resistance. Compromised by the king's indecisiveness, the regular army wavered, several units allowing themselves to be disarmed by the people. The legions of the Parisian National Guard remained passive when they did not join the insurgents outright. The only serious opposition was offered by the 3,000 men of the Municipal Guard, and this was quickly overcome. Urban support and rural acquiescence were so widespread that Louis Philippe was forced to flee Paris, dressed as an ordinary citizen, barely 48 hours after hostilities began.

Yet, while a consensus existed against the Orleanist monarchy, no comparable consensus existed in favor of a specific set of social and political institutions which might replace it. No sooner had France's Second Republic been proclaimed than latent social conflicts began to emerge. Over the succeeding months, contending parties attempted to redefine the institutional structure, operating symbolically through the distinction between

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the "democratic republic" of the moderates and the "democratic and social republic" of the radicals; and operating most concretely through the *journées* or revolutionary days of March 17, April 16, and May 15.

This progressive polarization culminated in the insurrection of June. The insurgent camp—perhaps 40,000 strong—was composed of a fraction of the working class disillusioned with the hesitant social policy of the republican government. Among them, members of the Parisian National Workshops played a pivotal role. The insurrection was opposed, and after four days suppressed, by a combination of military forces loyal to the government. These included the bourgeois legions of the National Guard (those from the working-class sections of the city abstaining from the fighting or joining the insurgents); about 30,000 regular army troops, who displayed no great enthusiasm in the early fighting; and, in particular, the 14,000 volunteers of the Mobile Guard who constituted the shock troops of the repression.

Two complementary myths, both of which can be traced back to Marx's account of the June Days in *The Class Struggles in France* (1964), had, until relatively recent times, won widespread acceptance in sociological and historical analyses of the period. The first is that the June insurgents represented the revolutionary *proletariat* of Paris. The second is that the Mobile Guard, the military force which spearheaded the June repression, was composed of *lumpenproletarians*. There exist, however, empirical grounds for questioning both these assertions and with them not only received interpretations of 1848 but a whole variety of approaches which seek to explain participation in social movements on the basis of class origin and occupational status.

In this paper, I first compare the class origins of participants on both sides of the June insurrection; second, I examine nonclass differences among individuals in each camp; and third, I focus on the *collective* characteristics of the organizations with which they were affiliated in an attempt to arrive at an explanation of opposing political orientations in the June Days.

CLASS ORIGINS OF THE JUNE COMBATANTS

Marx employed the concept of the "proletariat" in such varied contexts that it is impossible to provide a concise and consistent definition which exhausts its many meanings. The logic of the general theory—that the inexorable development of contradictions inherent to industrial capitalism leads to socialist revolution—clearly implies that unskilled, pauperized factory workers, newly created as a by-product of that development, would be the agents of revolutionary change. But when writing of the concrete historical events of 1848, Marx applies the term "proletariat" to a Parisian

lower class in which the process of proletarianization had made only limited progress. This ambiguous usage obscures the relationship between specific forms of economic development and the potential for specific forms of social movement activity.

Certainly the Parisian economy of 1848 was much less industrialized than one might anticipate from a reading of *The Class Struggles* alone. Despite the genuine economic growth realized under the Restoration and the July Monarchy, France remained industrially backward. Insofar as an industrial sector existed, it was concentrated in provincial cities like Lille and Rouen, while Paris, with its high rents and wages, remained the center for luxury trades employing skilled artisans in small workshops. Even the great spurt of industrialization in the first decade of the Second Empire left the situation so little changed in the capital that Brogan (1963, p. 153), writing of the Commune of 1871, could assert that "the Paris which drifted into revolt was not an industrial city of the modern proletarian type. As Sir John Clapham pointed out, it lacked the very basis of the new factory economy, power-driven machinery. There were true proletarians of the classical type in the railway yards; there were a few mass-production industries like the plate factories of the popular jeweller, Christophle. But Paris was a city of craftsmen, of shopkeepers, of servants, of officials, of clerks." If, as Marx wrote in 1856, "steam, electricity, and the self-acting mule were revolutionaries of a rather more dangerous character than even citizens Barbès, Raspail, and Blanqui" (Tucker 1972, p. 427), the prospects for socialist revolution in France in 1848 were certainly equivocal.²

The inquiry conducted in May and June of 1848 by the *Chambre de commerce de Paris* (1851) confirms the view that Parisian workers were typically well paid (their average salary of Fr 3½ was twice that of the rest of France) and highly trained (87% of all males were literate). It also shows that they worked in traditionally organized shops, only 11% of which employed more than 10 workers. The first column of table 1 shows the distribution of production workers among the 13 categories used in the *Chambre de commerce* study. Two sectors can be considered to have been in the process of industrialization. The manufacture of textiles had to some extent been mechanized; in the making of clothing traditional enterprises were experiencing competition from a minority of *tailleurs confectionneurs* or "ready-made" tailors (Johnson 1975). The figures show, however, that of the 200,000 male Parisians surveyed, the great majority worked in artisanal trades.

It might yet be thought that while the general Parisian labor force was

² "According to the economist Audiganne, in 1847 only 318 workshops in the department of the Seine used mechanical power or employed more than twenty workers. Only about 30,000 workers were employed in these, with countless more employed in smaller and more technically primitive workshops" (Price 1972, p. 6).

predominantly artisanal, the June insurgents were drawn selectively from its most proletarianized elements. The work of Tilly and Lees (1975) has demonstrated that this is not the case.³ Column 2, based on their data, shows that the distribution of insurgents across sectors of the Parisian economy closely resembled that of the general population. Furthermore, when these figures are used to calculate rates of participation per 10,000 male workers in each sector, we find that those in textiles and clothing, the most highly proletarianized sectors, were represented at or below the overall average rate.⁴ Thus, unsupported allusions to the June insurgents as the revolutionary proletariat of Paris are exaggerated at best.

TABLE 1
OCCUPATIONAL DISTRIBUTION BY SECTOR
(Chambre de commerce Categories)
(%)

SECTOR	POPULATION		
	Adult Male Parisians (1)	June Arrestees (2)	Mobile Guard (3)
Food.....	3.9	5.8	4.6
Construction.....	19.6	26.2	22.4
Furniture.....	14.0	9.9	12.0
Clothing.....	14.8	14.9	13.2
Textiles.....	5.4	4.7	7.2
Leather.....	2.1	2.4	1.0
Carriage making....	5.2	2.3	3.6
Chemicals.....	3.2	1.9	2.6
Ordinary metals....	10.8	17.6	12.8
Fine metals.....	5.3	3.4	5.6
Basketry.....	2.0	1.5	3.7
Luxury trades.....	8.6	2.9	4.0
Printing, paper....	5.3	6.5	7.1
Total.....	100.2	99.7	99.8

SOURCES.—Col. 1: *Chambre de commerce de Paris* (1851), *tableau général* no. 3, and pt. 1, pp. 49, 68; col. 2: Tilly and Lees (1975), pp. 188–89, tables 1, 2, based primarily on France, Archives nationales (F⁷ 2586); col. 3: my own (Traugott 1980) occupational classification of the Mobile Guard, based on France, Archives de l'armée de terre (Série X^m).

³ I have cited the work of Tilly and Lees (1975) in preference to that of Price (1972) in part because the former have provided an explanation of their coding system (Lees and Tilly n.d.) so detailed and explicit that it is readily replicable, and because they have investigated in greater depth the correlates of insurgent participation. Similarly, I use my own classification of Mobile Guard occupations (tables 1 and 2) in preference to that of Caspard (1974) because the former permits direct comparison with figures on the Parisian population (*Chambre de commerce de Paris* 1851) and the insurgents (Tilly and Lees 1975). The conclusions presented here would, however, be substantially the same if based on the data of Price or Caspard.

⁴ Tilly and Lees report rates based on *all* Parisian workers, presumably because 273 of 11,616 original suspects were women. As mentioned above, clothing and textiles were

But what of their June opponents, in particular the Mobile Guardsmen who earned the scorn of Marx and Engels for the enthusiasm with which they repressed the insurrection? No contemporary observer or subsequent analyst has questioned the fact that recruitment efforts—ironically aimed first at February insurgents—produced a corps in which members of the lower classes predominated. Less certain is the claim of Marx and Engels that these men represented the lumpenproletariat, that stratum of “thieves and criminals of all kinds, living on the crumbs of society, people without a definite trade, vagabonds, *gens sans aveu et sans feu* . . .” (1959, p. 298). Our data confirm the findings of Caspard (1974) that, far from being lumpenproletarians, members of the Mobile Guard were at least as literate and highly skilled as their counterparts among the insurgents. A glance at column 3 of table 1 reveals the same general correspondence with the city-wide occupational distribution for the Mobile Guard that we have already observed in the case of the insurgents. Still more remarkable is the convergence produced by a direct comparison of Mobile Guardsmen and those arrested in June. Note also that of the two most highly proletarianized sectors, clothing workers are proportionately distributed between the two camps, while textile workers were somewhat more likely to oppose than to join the insurgents.

Columns 1 and 2 of table 2, based on the extended classification scheme of Tilly and Lees, confirm this convergence not only among production workers but also among those employed in transportation, services, the professions, and commerce. The greatest difference is the unsurprising preference of military veterans for the militia. In short, all empirical evidence underscores the similarity in class origins between insurgents and Mobile Guardsmen and reveals references to the latter as “the organized lumpenproletariat” (Marx and Engels 1976, p. 142) to be simple distortions.

NONCLASS EXPLANATIONS OF POLITICAL ORIENTATION

Because class origins per se cannot account for the widely divergent political behavior of two groups so nearly identical in composition, we must

the most proletarianized segments of the Parisian economy. Not incidentally, they were the only sectors in which women were in the majority (64.4% and 66.5% of adults, respectively; the average for all other sectors was 18.3%). Thus, calculating rates of participation for an overwhelmingly male body of arrestees on a base which includes the sectorally concentrated group of women workers has the effect of artificially depressing rates in those two sectors. I have recalculated rates based on male workers only, a more conservative procedure in terms of the contention that proletarianized trades were not overrepresented among insurgents. I found that, of 13 sectors, textiles and clothing rank seventh and eighth, respectively, in rates of insurgent participation.

look elsewhere for an explanation of the June alignments. Two alternative lines of argument exist; I shall sketch one briefly here and develop the other at somewhat greater length in the next section. The first, following the lead of Caspard (1974), departs from the aggregate characteristics of June participants. It notes that the timing of recruitment for, and the conditions of service in, the Mobile Guard interacted with the economic and social characteristics of potential volunteers to produce a corps of men most obviously differentiated from the insurgents in that the respective average ages of the two groups were 22 and 34 years. There is no need to resort to psychologistic arguments about unformed minds in associating the youth of the Mobile Guard with the pliancy of its political behavior, for youth represents more significantly a marginality of status. Because the Guard was a full-time militia, quartered in barracks, it appealed almost exclusively to the unmarried. The bulk of its 16,000 members were recruited early in March as the level of unemployment, already critically high, rose precipitously as a result of the uncertainties created by the February Days themselves. The younger skilled workers who enlisted, though not inexperienced in the workplace (Caspard 1974, p. 90), were

TABLE 2
OCCUPATIONAL DISTRIBUTION BY SECTOR
(Tilly and Lees's Categories)
(%)

SECTOR	POPULATION		
	June Arrestees (1)	Mobile Guard (2)	National Workshops (3)
Food.....	4.1	3.7	.9
Construction.....	18.4	15.0	24.1
Furniture.....	6.0	8.1	10.0
Clothing.....	9.2	8.7	5.1
Textiles.....	3.1	4.9	4.7
Leather.....	1.5	.7	.7
Carriage making.....	1.7	2.4	1.4
Chemicals.....	1.3	1.7	.6
Ordinary metals.....	11.8	8.0	12.0
Fine metals.....	2.1	3.7	4.3
Basketry.....	1.2	2.4	3.8
Luxury trades.....	1.9	2.5	1.5
Printing, paper.....	3.9	4.7	3.2
Transportation.....	4.7	2.0	1.1
Services and others.....	14.7	14.1	25.3
Professions, finance, students....	2.8	2.2	.1
Retail trade.....	7.1	6.6	1.2
Military.....	4.4	8.5	.1
Total.....	99.9	99.9	100.1

SOURCES.—Col. 1: Tilly and Lees 1975; col. 2: Traugott 1980; col. 3: my own classification of the National Workshops based on Thomas (1848).

those in the most vulnerable economic position. In the short-term crisis occasioned by the February overturn, they had typically been the first to be let go; in the long-term crisis faced by artisanal trades in a gradually industrializing economy, they had the poorest prospects for mobility and security. In contrast, insurgents tended to be longer-established practitioners of their trades, immersed in family, neighborhood, and professional networks to a greater extent. Though Mobile Guardsmen and insurgents derived from a common class base, the fact that they constituted different age cohorts within a contracting economy produced significant differences in political orientation. Older insurgents mobilized collectively against a government which had reneged on its promise to rescue and rebuild an economic system to which they were deeply committed. Younger Mobile Guardsmen defended the republic which had furnished them with public employment more secure than any they could expect to find in the private sector.

This explanation, which might be labeled the "cohort hypothesis," revolves around an ambiguous interpretation of a difference in age. Not only is it difficult to disentangle the various correlates of age (work experience, economic vulnerability, marital status, etc.) which may have guided a process of self-selection among Mobile Guardsmen and ultimately influenced their political behavior in June; it is also quite plausible that the difference in age is a merely artifactual consequence of externally imposed selection factors. The decree establishing the Mobile Guard confined recruitment to those 16–30 years of age. Though often loosely applied, these age restrictions provide a simpler explanation of the only substantial aggregate difference between the groups which fought on opposite sides of the June barricades.⁵

THE "ORGANIZATIONAL HYPOTHESIS"

Instead of seeking an explanation in variations imputed through aggregate characteristics of individual members of acting collectivities, we might also consider the characteristics of those collectivities themselves. By juxtaposing two institutions of the early Second Republic—the Mobile Guard and the National Workshops—we can focus on the organizational structures and experiences to which June participants were exposed in the

⁵ It should be possible to determine whether the imputed causal factors represented by age were in fact efficacious by establishing the average age of the approximately 150 Mobile Guardsmen arrested in June among the insurgents. If arrested Mobile Guardsmen more nearly resemble other insurgents in age, the case for the importance of age-related differences is strengthened. If, however, they more nearly resemble loyal Mobile Guardsmen, the case is weakened. These data are currently being collected as part of ongoing research.

months preceding the final confrontation.⁶ The careers of these institutions were roughly parallel. Both were created in the days following the February revolution for essentially the same purpose: to quiet and control the volatile mass of unemployed Parisians. Both were patterned on a military model and figured importantly in events leading to and including the June insurrection. A review of the role played by these two organizations between February and June will place in relief the relevant points of comparison.

In that period, three major popular disturbances occurred. The first involved the twin demonstrations of mid-March. On March 16, the more conservative elements of the Parisian National Guard marched in protest of the disbanding of the elite companies. This was answered on March 17 by a gathering of 100,000 Parisians in favor of the policy of National Guard democratization and the postponement of upcoming elections. These essentially peaceful attempts to influence government policy, staged less than three weeks after the founding of the republic, may well have marked the high point of progressive support. What parts were played in these events by our two focal groups?

The Mobile Guard was created as a full-time militia which could replace the discredited social control forces of the deposed monarchy. Enlistments were opened in the last days of February, but for several weeks the resolution of personnel and practical problems and the instruction of recruits remained the order of the day. By mid-March most battalions had barely reached half strength and were just beginning to arrange for the election of officers. Thus the Mobile Guard was unprepared for the eventualities of March 17 and played no active role.

The National Workshops, though of similarly recent date, had grown more quickly. The Provisional Government had intended to provide temporary employment in public works projects for a portion of the Parisian labor force left destitute by the economic crisis. But contrary to the government's belief that provision for 10,000 workers would prove largely sufficient, in just the first week of enrollments 14,000 of the unemployed had signed up (McKay 1933, p. 159). Director Emile Thomas, warned of the leftist agitation scheduled for March 17, was determined to keep the entire membership occupied even though he had work enough for only 1,000. Some of his workers protested that the administration, which had torn down signs publicizing the march, was attempting to isolate them from

⁶ The National Workshops cannot, of course, be identified with the insurgent cause. Since insurgent strength is usually estimated at 40,000-50,000, and since workshop enrollments had swelled by June to between 100,000 and 120,000, it is clear that only a minority of the latter could have fought on the barricades. It is nonetheless true that the National Workshops contributed a very significant share of the June arrestees (43% according to Tilly and Lees [1975, p. 94]) and that this organization constituted the center of recruitment and mobilization for the insurrection.

Parisian politics (Thomas 1848, pp. 95–98). If so, the effort was successful. Members reported to workplaces dispersed throughout the city, so that Thomas, in responding later to allegations made by the commission of inquiry on the June insurrection, could honestly claim that for the day of March 17 his men had a “perfect alibi.” Thus, while in this earliest confrontation many members of the National Workshops had a decided sympathy for the leftist cause, the administration had managed to deflect them from overt action.

On April 16, leftists protested the government’s failure to pursue energetically a program of social welfare. Their primary demand was for the “organization of labor,” but they also hoped to make a show of strength which might favorably influence the impending elections.

This time the Mobile Guard was present in force. Called upon to control an unruly crowd of demonstrators, the response of these previously untried troops whose loyalty was so much in question proved unhesitating. So pleased was Commanding General Duvivier that he issued a statement to his men congratulating them on their satisfactory performance (Chalmin 1948, p. 57).

One might have expected the demand for the “organization of labor” to attract a sizable share of the 65,000 workers who had enrolled in the National Workshops by April 16. It is true that large numbers arrived with their company banners at assembly points on the Champ-de-Mars and at the Hippodrome. But when representatives of the central administration were dispatched to these staging areas, they succeeded in dissuading most members from participating in the actual march on the Hôtel de Ville. Only a portion of the Hippodrome group continued on, and though McKay (1933, p. 51) sees this as a first, tentative sign of shifting workshop loyalties, the overall impression is that again in April the organization’s progressive wing had rather effectively been held in check.

The *journée* of May 15 began with the stated objective of petitioning the National Assembly concerning French policy toward Poland but quickly developed into an attempt to intimidate that newly elected body’s conservative majority. Once the demonstrators, only loosely controlled by their leaders, had crossed the Seine, they proceeded to flood the observation galleries, infiltrate the floor, seize the podium, and finally declare the assembly dissolved.

The invaders managed to gain entry to the assembly chambers despite the deployment of forces under National Guard Commander Courtais. Tempoure, the new commanding general of the Mobile Guard, and several battalions of his troops were subordinated to Courtais. The latter, at the moment of confrontation with the demonstrators, failed to beat the *rappel*, ordered the Mobile Guard to sheath its bayonets, and thus allowed the sovereign political body of France to be taken without resis-

tance. General Tempoure, subsequently escaping from the assembly where he had been taken prisoner, rallied a group of Mobile Guardsmen whom he found nearby and succeeded in sweeping the insurgents from the area, holding his ground until reinforced by additional National Guard units. In May, the initiative and loyalty of the Mobile Guard saved the republican government from the most serious threat it had thus far encountered.

Very few members of the National Workshops were among those expelled from the Palais Bourbon. Fourteen thousand had gathered at the Place de la Bastille to begin the march, but by the time the procession reached the Pont de la Concorde, where the National Guard and Mobile Guard troops were stationed to defend the approach to the assembly, most members of the workshops had withdrawn. Here again the urgings of the directors were instrumental. So much so, Thomas estimated, that three-fourths of the National Workshop members originally assembled ultimately heeded the beating of the *rappel* and participated as members of their National Guard units in freeing the National Assembly from invaders.

What this admittedly elliptical account shows is that the behavior of both focal groups in the preliminary tests of strength contradicted the prevailing expectations of outsiders. As early as March, fears were expressed by members of the Provisional Government that the National Workshops could become a breeding ground of discontent and a weapon directed against the regime which had created them. Quentin-Bauchart, who served as *rapporteur* for the Commission of Inquiry on the June Days, wrote that "one had either to be simple in order not to sense, or blind in order not to see that the idlers of the National Workshops belonged, in every case, to the rising" (1901, p. 29; my translation). Despite the contrary evidence of their previous behavior, by June all contemporary observers assumed that the workshops would support a full-scale workers' insurrection.

The expectation that the Mobile Guard would similarly defect was widely held on both right and left and was not without foundation. At an April 5 meeting of the Club de la Garde nationale mobile, members had been asked what course of action was appropriate if "the National Assembly pursued a static program and did not extirpate in a radical manner all abuses." Their unanimous reply was, "Insurrection! insurrection being in this case the most sacred and holy of duties" (France, Archives nationales, C930, no. 570; my translation). The workers themselves were said to be counting on the Mobile Guard (Stern 1862, p. 381) and stepped up fraternization efforts which, according to a prefect's report dated on the eve of the June insurrection, were meeting with appreciable success (France, Archives nationales, C930, nos. 698, 689). So hopeful seemed the signs of Mobile Guard sympathy that, when the first telegraphic accounts of the June rising reached Engels, he was quite prepared to publish the erroneous report that the eleventh battalion of the Mobile Guard had

gone over to the insurgents (Marx and Engels 1976, p. 135). Tocqueville, the one social analyst to have sensed the coming of the February revolution and a qualified supporter of the government it brought to power, doubted the Mobile Guard's intentions so strongly that even after the event he insisted that its adherence to the cause of order in June had depended on the most tenuous of circumstances (Tocqueville 1971, pp. 162, 199).

What the historical record makes clear is that these groups, each of which had mixed political loyalties, initially resolved their ambivalences in favor of the constituted authorities or at least against active participation in the preliminary assaults on the republican government. Yet in June we find the National Workshops at the center of the insurgent camp, while the Mobile Guard emerges as the staunchest defender of the regime in power.

We can begin to make sense of this situation if we recall that both these institutions had adopted a quasi-military organization: the Mobile Guard by its very nature as a full-time militia; the National Workshops as the result of Thomas's proposal for restructuring⁷ (Lalanne 1848, 3:1; McKay 1933, pp. 20-23). The factors contributing to the loyalty or disloyalty of a military formation to a political regime in revolutionary situations have been analyzed by Chorley. Her principal conclusions are the following: (1) no popular insurrection can succeed against the energetic opposition of trained troops; (2) except at the end of an unsuccessful large-scale war, the rank and file will invariably obey a unified corps of officers who, if properly socialized, will support a status quo regime; and (3) even where the officer corps is divided or uncertain, the rank and file is likely to be won over to a revolutionary cause only to the extent that the length of service is short, their segregation from the civilian population breaks down, and practical grievances are allowed to persist without redress (1943, pp. 175, 181, 242-48).

These generalizations help explain the behavior of the Mobile Guard. Its general staff and a substantial share of its officer corps consisted of professionals transferred from the army of the line. Most officers were elected by the rank and file, but the troops chose those with previous military experience in overwhelming proportions: 59% of all lieutenants, 78% of all captains, and 100% of all battalion commanders (France, Archives de l'armée de terre, Vincennes, Série X^m). Thus, the officer corps of this supposedly all-volunteer militia was far less "democratic" than might be

⁷ One reason for the enthusiasm with which Marie, minister of public works in March, greeted Thomas's proposal for the quasi-military organization of the National Workshops may have been that he "envisaged the possibility of employing the Workshops' organization as an army to combat the radical proletariat in the eventuality of a further insurrection" (McKay 1933, pp. 40-41).

supposed and, especially at the highest levels, remained unswervingly loyal to the increasingly conservative government.

The officers' steadfast adherence countered any tendency to disaffection arising from the fact that the rank and file consisted of short-term volunteers.⁸ Also important was the fact that Mobile Guardsmen were quartered in the barracks left free by the withdrawal of regular troops from Paris and thus were fairly effectively isolated from daily, intimate contact with the civilian population. But the handling of practical grievances provides perhaps the best illustration of how the development of Mobile Guard political orientation was a dynamic process influenced by the actions of contending parties.

It was inevitable that an organization established in such haste should encounter difficulties in quartering, provisioning, and arming its members, but the administrative failure which most frustrated the rank and file appears to have been the inordinate delays involved in procuring Mobile Guard uniforms.⁹ The effect on morale was so troubling that in Commanding General Duvivier's congratulatory message of April 18, he was careful not only to praise his men for their loyal conduct but also to secure their continued good will with the promise that the long-awaited uniforms would arrive presently. As Chorley (1943, pp. 244) points out, the great weakness of practical grievances as a spur to revolutionary action by military units is that they can be quickly remedied by an attentive government. In effect, the authorities, recognizing their vulnerability, hastened to make special arrangements with the Fraternal Association of Tailors to expedite delivery. The level of Mobile Guard disaffection declined rapidly once uniforms began to arrive late in April (France, Archives nationales, C940, no. 7293). Thus, the loyalty of this predominantly lower-class organization was secured through control of its officer corps, the isolation of its rank and file, and prompt attention to its basic needs.

The National Workshops present a more complex case. The occupational composition of the membership (table 2, col. 3) was roughly equivalent to that of the Mobile Guard and the insurgents, though it appears to have been more closely tied to sectoral variations in the rate of unemployment. Moreover, members of the workshops originally earned the same

⁸ Enlistees signed on for one year and a day, a length of service intermediate between that of the regular army and a true militia.

⁹ While the question of uniforms may seem trivial to sociologists concerned primarily with the grand sweep of history, this issue reappears so insistently in the political developments of 1848 that it seems to merit attention. From identification of the elite National Guard units demonstrating on March 16 as *bonnets à poil* to the ostentatious manner in which Caussidière outfitted his radical *montagnard* police force, styles of dress carried symbolic significance. McKay (1933, p. 45) says of the Mobile Guard, "Uniforms, good pay, and barrack-life contributed to the rapid development of esprit-de-corps and to the weaning of this group from its proletarian sympathies."

wage offered in the Mobile Guard. But since available work projects never occupied more than a tiny minority of enrollees, these payments assumed the character of a demeaning dole. Workers' expectations of their prospective "length of service," shaped by the hope that the economy would revive and the fear that the workshops would be dismantled by a bourgeois government, remained uncertain. The very nature of the organization precluded the strict segregation from the "civilian population" of members who returned to their families and to working-class neighborhoods each evening. Until late May, the Thomas administration made energetic and generally successful efforts to insulate members from the influence of the radical supporters of Louis Blanc's Luxembourg Commission¹⁰ and to dissuade them from participation in the three major *journées*. These "victories," however, were won under circumstances—specifically, the uncertainty over the workshops' continued existence, members' constant immersion in street politics, and practical grievances concerning the lack of useful work—which only a forcefully organized administrative structure could counteract.

This forceful organization had been laid out by Emile Thomas, a 26-year-old former student of the Ecole centrale des arts et manufactures. He earned the directorship by rescuing the Provisional Government from its embarrassing lack of ideas about how to make good on its promise of "the right to work." He formed the upper echelons of the workshop administration by recruiting friends and acquaintances among students and alumni of his alma mater. Lower echelons were elected by the membership, often from among the individuals who had played an active role in the recruitment of the rank and file (Lalanne 1848, 4:2). The bulk of this supervisory personnel was firmly committed to the "democratic republic" and to a policy of social peace through social welfare efforts. The access of the cadres to the rank and file was insured in part by the quasi-military organization of members into squadrons, brigades, companies, and so forth; and in part by the creation of a "club of delegates" as a forum for the process of cooptation. For nearly three months, this carefully selected and organized administrative structure was able to exercise a decisive moderating influence over workshop members. Yet this pattern of moderation was sharply reversed in June. What happened in the interim that explains the sudden shift in political orientation in the National Workshops and their leading role in the leftist insurrection?

The answer lies in the brusque removal, under unusual circumstances, of Emile Thomas and the consequent disruption of the administrative structure he had established. Thomas had served the government faith-

¹⁰ McKay (1933, p. 40) describes Thomas as a "firm friend of the cause of order and implacable opponent of Louis Blanc and all his works."

fully and well, but always out of a sincere concern for the welfare of the workers under his direction. When informed on May 24 of plans for the reduction and eventual elimination of the National Workshops, he hastened to the office of Trélat, the new minister of public works, where he was told: "You must help us to destroy what you created; what formerly was necessary has today become dangerous" (Thomas 1848, p. 285; my translation). Thomas objected that this action was contrary to the spirit of the February revolution as well as to specific assurances made to the workers and would create the risk of serious unrest. Although he expressed his reluctant willingness to obey the orders of his superior, his profession of loyalty seems not to have been convincing. On May 26, he received a letter summoning him immediately to the minister's office. Once arrived, he was unceremoniously asked for his resignation. Thomas directed at Trélat one of those appeals for moderation which had proved so effective with the workers. It was rejected out of hand. Thomas drafted his resignation, whereupon Trélat informed him that he had been assigned a new "mission," left unspecified, which required that he leave instantly for Bordeaux under armed police guard without the opportunity of communicating with friends or relatives. The director of the National Workshops had been exiled from Paris.

Since May 15, the government had lived in fear of a lower-class insurrection. In dismissing Thomas, it made a self-fulfilling prophecy of its conviction that the National Workshops would turn against it. This act alienated the administrative personnel, many of whom retained intense ties of personal loyalty to Thomas, and thus removed the barrier which had until then restrained the radicalism of the rank and file. The morning of May 27, upon learning of the circumstances surrounding Thomas's departure, his five assistant directors (one of them his brother) tendered their resignations. That same day, the supervisory personnel of the workshops lodged a vigorous protest with members of the Commission on Public Works. At a hastily convoked meeting of the club of delegates, Trélat's evasive replies to pointed questions provoked such hostility that the minister had to be protected by the assistant directors and led from the assembly hall by a back door. From May 27 on, insurgency among the rank and file grew rapidly (France, *Assemblée nationale constituante* 1848, 1:242). Members petitioned the government, assembled in the streets, and sought a rapprochement with the radical remnants of the Luxembourg Commission. This reaction forced a brief postponement of the initial plan to curtail the National Workshops but only confirmed the government in its resolve to do away with this "nest of disorder." The announcement three weeks later of the first phase in the dissolution of the workshops was the immediate precipitant of the June insurrection. It was in the workshops that the demonstrations of June 21 and 22 were organized. Workshop ban-

ners were planted atop the first barricades. Workshop cadres assumed the leadership role in much of the fighting, while workshop members constituted nearly half of those later arrested. Between May 26 and June 23, new lines of battle had been drawn, and the National Workshops now belonged to the insurgent camp.

CONCLUSIONS AND IMPLICATIONS

The following propositions summarize the conclusions emerging from our examination of the revolutionary situation in Paris, 1848.

1. Participants on both sides of the June fighting were remarkably similar in terms of class origins. Neither lumpenproletarians nor true industrial proletarians, they constituted instead a broad cross section of a predominantly artisanal economy experiencing short-term crisis and long-term decline.

2. Organizational characteristics affected differentially the evolution of political orientation in the two camps between February and June. Specifically, in the Mobile Guard an officer corps dominated by professionals retained its coherence and managed to a considerable degree to isolate the rank and file from the civilian population, while the government responded opportunely to the most serious grievances among the common soldiers. The administration of the National Workshops was similarly successful in controlling and coopting workers through mid-May. Only after the government shortsightedly intervened, removing the popular director and alienating his subordinates, did the sympathies of the National Workshops shift definitively in the direction of militant action.

These considerations justify the view that in June 1848, the decision to support or oppose the constituted government in the actual fighting seems to have depended not on class origins as such but on a combination of structural and situational factors which are best placed in relief by historical and organizational analyses of the acting collectivities.

Two general implications for research on social movements can be drawn from my analysis of 1848. The first concerns the hidden assumptions of, the second the appropriate time frame for, relating characteristics of participants to characteristics of the movement with which they are associated.

First, a tradition of class analysis, exemplified by approaches as diverse as those of Marx (1964), Rudé (1961), and Moore (1967), seeks to illuminate the causes, objectives, and historical significance of revolutionary protest by establishing the predominant class affiliation of those taking part. At its simplest, the argument is the following: to know the class origin of participants is to know their consciousness and motivation and thus

to understand the purpose and import of the movement itself.¹¹ While this type of explanation is of proven value, it also has its limitations. One is that simple categorical reasoning cannot explain the degree of internal heterogeneity which often exists within insurgent formations, much less the great singularity of the June insurrection, in which opposing sides were practically identical with respect to socioeconomic status. Class analysis, most valuable in the interpretation of long-term secular trends where situationally specific variations can be expected to wash out, is less pertinent to the explanation of concrete revolutionary episodes. A second limitation is that, for the assumptions of class organization to be met, it must be possible for conflict to be expressed organizationally. In 1848 the Parisian working class did not possess the means of concerting its political influence or coercive potential. In the absence of a class-based organizational framework, the efficacy of class as a determinant was diminished. The findings above suggest that in comparably fluid stages of class formation, collective variables other than class—for example, the nature of authority relations in existing organizations, the degree of isolation of mobilized constituencies, and the response of the institutional structure to their grievances—can provide the necessary explanatory leverage. Since all the major, successful revolutions of the modern era occurred in just such transitional societies, this seemingly trivial point assumes crucial significance for class-oriented theories of revolution.¹²

The second general lesson is that analysis based on a simple cross section in time fails to capture the dynamic quality of the process by which collective action is shaped. A static view of members of the Mobile Guard and National Workshops at the moment when these organizations were formed in early March would reveal virtually identical political orientations and overlook the potential for divergence. A static view in late June would reveal sharp differences in political orientation and overlook the remarkable malleability to which the developments of the prior three months bear witness. It is the analysis of the intervening changes in the internal structure of acting collectivities and of the interactions among contending groups which permits us to make sense of their contrasting evolutions. This implies that revolutionary conflict must be seen as a dynamic social

¹¹ At its crudest, this reasoning proceeds from social cause to social effect through individual states of mind, often imputed without direct evidence. In this form it can resemble social-psychological theories (e.g., Davies 1962; Gurr 1968) which connect social causes and effects through imputed individual reactions to the experience of deprivation, for which empirical confirmation is rarely available.

¹² I might, in fact, be argued by extension that where the means exist for self-conscious class expression of deep-rooted social conflict, the probable outcome is *non*revolutionary. The work of Paige (1975), e.g., supports such a conclusion in the agrarian context.

process in which collective forces determine outcomes progressively, working through but not rigidly bound by structural conditions which require generations or centuries for their complete elaboration. These are the implications which the case of France in 1848 calls to the attention of students of revolutionary social movements.

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Physiological Aspects of Communication via Mutual Gaze¹

Allan Mazur

Syracuse University

Eugene Rosa

Washington State University

Mark Faupel

University of Georgia

Joshua Heller and Russell Leen

Syracuse University

Blake Thurman

Hobart and William Smith Colleges

Eye contact between two people often serves as a channel of nonverbal communication. It may signal aggressive or dominating intent, as in a staredown, or it may convey an impression of close bonding, intimacy, or a request for aid, depending on how actors define the situation. Some theories of nonverbal communication assume that mutual gaze is physiologically arousing and that this arousal accounts in part for the efficacy of eye contact as a communication channel. The first experiment reported here shows that mutual gaze does indeed cause more physiological arousal than control conditions of nonmutual gaze. The second experiment demonstrates that an actor can communicate with another person by gazing in such a manner that he can manipulate that person's physiology. A third experiment demonstrates that a subject's response to a mutual gaze is a good predictor of his or her degree of influence (dominance) in subsequent social interaction. These results emphasize the rapid emergence of status differentiation in face-to-face interaction between strangers.

We have taken a simple and common social situation, mutual gaze between two people, and examined physiological responses throughout the interaction. We will show that mutual gaze produces a different physiological response from unreciprocated gaze, and that an actor can communicate with another person by manipulating the intensity of this response through "eyebrow signaling" (Keating, Mazur, and Segall 1977). Furthermore, subjects' responses to mutual gaze provide a good prediction of their degree

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of influence (dominance) in subsequent social interaction. These results explain important aspects of nonverbal communication through the eye contact channel, particularly communication relevant to the formation of status or dominance hierarchies in small groups. Our method is neither traditional nor widely accepted in sociology, but therein lies its virtue, for it provides a new way of looking at commonplace social behavior.

The act of staring or gazing at another person can take on a variety of meanings depending on how the starrer and the recipient of the stare define the situation. The stare is commonly interpreted as an assertive gesture in a wide range of cultures (Watson 1970), and it serves as a display of dominance in all nonhuman primates which have been studied (Jolly 1972; Chevalier-Skolnikoff 1973). Several empirical studies demonstrate the threatening and dominating character of stares (Moore and Gilliland 1921; Thayer 1969; Exline, Ellyson, and Long 1975; Ellsworth 1975; Liebow 1967; Fromme and Beam 1974); however, there are situations in which staring is likely to be defined as a sign of close bonding or as a request for aid (Simmel 1921; Rubin 1974; Goffman 1963; Ellsworth 1975).

Our concern with physiology is based on the common notion that many emotions and forms of motivation have visceral substrata, a point amply documented in the literature of physiological psychology. At one time, it was thought that each emotion had a characteristic pattern of somatic effects, but this view has not received much empirical support. Theoretical opinion has shifted to the view that the same pattern of visceral arousal may serve as the physiological basis of several different emotions, the particular emotion that is experienced being identified through cues in one's environment. This view gained considerable support from a classic experiment by Schachter and Singer (1962), who were able to induce two different emotions, anger and euphoria, in subjects experiencing physiological arousal from injections of adrenaline. The particular emotion induced depended on cues received by the subjects from an experimental confederate who acted out either a happy or an angry role, depending on condition. It would be incorrect to conclude that physiology is irrelevant to emotions. Adrenaline arousal was a prerequisite for eliciting both emotional responses. Furthermore, some attempts to correlate particular emotions with particular physiological states have produced positive relationships (Funkenstein, King, and Drolette 1957). Thus, our present state of knowledge indicates an intimate but complex (and probably indirect) link between cognitive and visceral elements of emotion and motivation. If we wish to explain social interaction in terms of the motivation of individual actors, as in exchange theory (Homans 1961; Blau 1964) for example, we should examine physiological, as well as cognitive, elements of motivation, since both are important and they do not have a one-to-one correspondence.

Undoubtedly, there are areas of social discourse that have little to do

with emotional or gut-level motivation. But many other areas are deeply touched by feelings of love, envy, anger, fear, lust, hunger, anxiety, disgust, or euphoria. One such area which has been a particular concern to us is the distribution of "status" (defined as ranking on the basis of power, prestige, and influence) among members of small groups. Strongly influenced by the dynamics of status hierarchies among nonhuman primates, we envision a similar process among humans. It is common among primates to establish and maintain a status hierarchy through a series of face-to-face encounters among the members of the group. At one extreme, these encounters involve fierce combat to determine the victor and vanquished. At the other extreme, very mild signals exchanged between two animals, perhaps a look or an eyebrow movement, may establish which is to be the more dominant. A mechanism postulated to operate across this range is the manipulation of discomfort levels during these encounters. An exchange of threats or attacks is seen as an attempt by each animal to "outstress" the other by inducing fear, anxiety, or pain. The animal that outstresses his adversary is the winner. When animals are unevenly matched at the outset, as when one is obviously more powerful or the other has a very low anxiety threshold, a simple stare by the powerful animal, followed by eye aversion by the submissive animal, suffices to establish status placement.

At first glance, this model seems inappropriate for humans, who rarely form status hierarchies through overt agonistic means. In normal everyday interaction, our feelings of comfort and discomfort fluctuate to a barely perceptible degree. However, it is possible that the emotional distribution of status occurs among us on a more subtle level than among the other primates. Chimpanzees and gorillas, the primates most closely related to humans, are much more subtle in status behavior than our more distant monkey relatives (Mazur 1973). Even more subtlety among humans would be a simple extension of this evolutionary trend.

The model becomes clearer if we consider a concrete example. Consider two strangers, Ego and Alter, whose eyes meet, by chance, across a room. Let us say that one of the strangers, Ego, decides to hold the stare. The chance eye contact now becomes a dominance encounter. Ego's stare makes Alter uncomfortable. Alter may then avert his eyes, thus relieving his discomfort while in effect surrendering, or he may stare back, making Ego uncomfortable in return. In the latter case, the staredown would continue with each person attempting to outstress the other until finally one person succumbed to the discomfort (and to the challenger) by averting his eyes. Many dominance encounters are even more subtle than a staredown, but they may follow similar dynamics.

We recognize the highly conjectural nature of this model and acknowledge that our experiments do not provide a test of it. We have presented these ideas in order to explain the conceptual basis for our work. The model im-

plies that mutual gaze (as well as other forms of communication which may be status related) should cause psychic and physiological effects which impact on motivation. Knowledge of such effects should allow us to predict subsequent status rankings. The experiments reported here were designed to explicate these physiological aspects of communication via mutual gaze.

PAST STUDIES OF STARE PHYSIOLOGY

Many theories of gaze interaction assume that a stare is physiologically arousing and that this arousal accounts in part for the efficacy of eye contact as a communication channel (Ellsworth 1975; Argyle and Cook 1976). Wada (1961) has shown that brain-stem responses of macaque monkeys are altered during eye contact with humans. Several studies of human subjects have examined galvanic skin response (GSR), heart rate, and EEG responses to staring. We will briefly review these results since they form the basis for our first experiment.

McBride, King, and James (1965) found more GSR activity when subjects stared at a confederate's eyes than when they stared at the confederate's mouth; however, the difference was not significant. Nichols and Champness (1971) had subjects gaze steadily at a confederate who alternately returned the gaze and averted his eyes. There was significantly more GSR during mutual gaze than during eye aversion, but at least part of this difference may be due to an order effect. Mutual gaze preceded eye aversion, so habituation of GSR could account for the lower activity during the later than the earlier condition.

Gale et al. (1972) had subjects repeatedly gaze steadily at a confederate who alternately smiled, returned the gaze (without smiling), and averted his eyes. Subjects' EEGs during mutual gaze were significantly different from EEGs during eye aversion, but this effect was found in only two of 10 frequency ranges which were examined. These differences may be due partly to habituation of the EEG response since, in the first trials, the mutual gaze condition preceded the eye aversion condition.

Klinke and Pohlen (1971) paired subjects with a confederate in a Prisoner's Dilemma game; in one condition of play the confederate gazed steadily at the subject, while in a control condition the confederate averted his eyes. Subjects under steady gaze had significantly higher heart rates than those in the eye aversion condition. Unfortunately, the subjects who received steady gazes were given no rationale for the confederate's unusual behavior, so their heightened heart rates may be due to the strangeness of the confederate's behavior rather than his gaze per se.

Each of the above studies suggests that the stare is an arousing stimulus; however, none is decisive. None except Gale et al. (1972) shows changes in the physiological variables over time. Thus, if stare responses occur, they

cannot be attributed to the initiation of eye contact as opposed to, say, the prolongation of eye contact. Furthermore, none of the studies is sufficiently controlled to distinguish among the effects of (1) participating in mutual gaze, (2) staring at a person who does not stare back, (3) receiving a stare and not returning it, and (4) staring at an inanimate object. To correct these deficiencies, we examined changes over time in GSR, pulse rate, and thumb blood volume (TBV) during a stare. The TBV had not been used in earlier studies, but it is a convenient variable that is known to react to stressors. In the typical "fight or flight" response to a stressor, blood is diverted from the periphery of the body, including the thumb, to the heart, lungs, and large muscles. Thus a stress response is indicated by a decrease in TBV (Vander, Sherman, and Luciano 1970; Bloom, Houston, and Burish 1976).

EXPERIMENT 1: FOUR STARE CONDITIONS

If mutual gaze acts as a stressor on the people staring at one another, then the gaze should be accompanied by a decrease in TBV. Increased pulse rate is also a feature of the fight-flight response, and on this basis we would expect mutual gaze to accelerate the pulse, as in Kleinke and Pohlen's (1971) tentative result. However, the Lacey's empirical results suggest that a person concentrating on an external stimulus (as when staring at a target) will show pulse deceleration. (For opposing views on this issue see Hahn 1973; Elliot 1972, 1974; Lacey and Lacey 1974.) Therefore, theoretical expectations for pulse change are unclear. Mutual gaze should show more GSR than the conditions of eye aversion, as in studies cited above. Our first experiment tests these hypotheses.

In order to separate the effects of mutual gaze from those of unreciprocated gaze, subjects were run in the following four conditions: (1) the experimenter (E) and subject (S) stared directly at each other; (2) S stared at E, who averted his eyes; (3) E stared at S, who averted his eyes; (4) E and S both stared at inanimate objects in the room.

Setting.—The experiment was run in a 10 × 18-foot office of the Sociology Department which served as a lab. Its decor was undistinguished except for the presence of physiological recording instruments. The lab layout appears in figure 1. Three young male graduate assistants served as Es; they dressed casually and tried to be relaxed with Ss.

Subjects.—Through campus ads, 80 undergraduates (45% male) were recruited; 56% volunteered, and the rest were paid \$2.00. Except for the constraint that they not know each other, Ss were randomly assigned to Es. Each E ran one-third of the Ss. Conditions 1 through 4 contained 22, 19, 21, and 18 subjects, respectively, though *N*'s in some data displays are slightly lower owing to equipment failures.

Procedure.—First, S was seated in chair 2 (fig. 1), and the study was

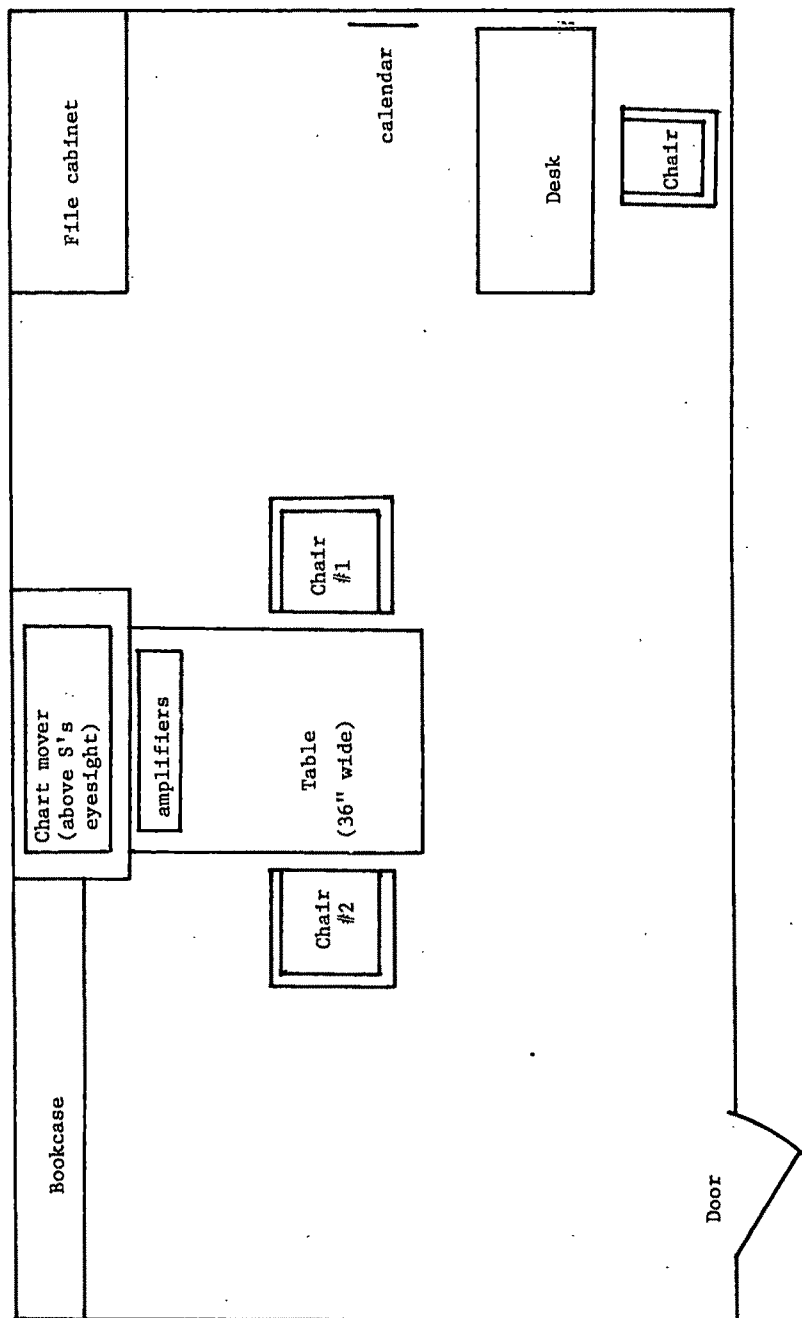


FIG. 1.—Layout of laboratory

explained as research into physiological correlates of staring. Then E attached physiological sensors to S's left hand. During a five-minute habituation period, E chatted with S and completed details of the record. Toward the end of this period, E drew a random number to determine in which of the four conditions S would be run; this procedure minimized E's opportunity to bias the outcome through his interaction with S. At the end of the habituation period, E sat in chair 1 and said:

Please keep conversation to a minimum now, and I would like us to avoid looking at each other unless I specifically say to do so. I'm going to start my [stop] watch again and let some time go by. After a while, I will say the word "now." When I do, then stare straight at [depending on the randomly-assigned condition]:

1. my eyes [E would stare back at S];
2. my eyes [E would stare away at an amplifier];
3. the calendar [E would stare at S];
4. the calendar [E would stare away at the amplifier];
until I say "look away." Please don't speak and try not to smile. I'll start my watch now.

After 30 seconds E said "now"—attempting to avoid a startled tone—to signal the beginning of the 20-second stare. Physiological responses were recorded for an additional 40 seconds after the stare. The sensors were detached, and S was debriefed.²

*Physiological variables.*³—Rather than measure GSR, we took its time derivative (DGSR), which is a clearer indicator of electrodermal activity (Rudin et al. 1971). Values for DGSR, pulse rate, and TBV were calculated for each consecutive 10-second interval. These intervals began 30 seconds before the stare (a baseline period), continued through the stare, and concluded 40 seconds after the stare had ended.

Changes in the width of the pulse rate track, as it appears on the chart paper, indicate changes in TBV in arbitrary units. We standardize TBV by assigning a value of 1.00 to its median width in the first 10-second interval of the baseline period; TBV in the n th interval is then the ratio of the median width of a pulse track in the n th interval to its width in the first interval. The third variable, DGSR, was measured as the maximum pen deflection in

² We attempted to evaluate the success of the manipulations by asking each S, during debriefing, where E was looking during the stare. If S said that E's eyes were averted in conditions 1 and 3, or that E stared at S in conditions 2 and 4, we scored an "incorrect" response, though these may simply be errors in recall. There were 2, 0, 1, and 9 incorrect responses in conditions 1 through 4, respectively, suggesting a problem in condition 4. Comparisons of subjects in condition 4 who gave incorrect and correct responses showed no substantial differences, so all subjects were retained.

³ All physiological instrumentation was manufactured by Harvard Apparatus Company. The DGSR was measured by electrodes on the first and third fingers of the left hand using a Model 385 GSR amplifier. Pulse rate and, indirectly, TBV were measured by a piezoelectric crystal pressure sensor (Model 361) held by a cuff to the left thumb nail; signals were fed to a Model 2162 crystal amplifier. Pulse rate is accurate to about 10%. Amplified signals went to pen drives mounted on a chart mover which was equipped with a timer/event marker (Rudin, Foldvari, and Levy 1971).

each 10-second time interval; it was standardized on the first interval in the same manner as TBV.

The data reducer was blind to the theoretical issues of the study.

Results.—We will begin our examination of the physiological variables by looking at similarities in trends over time across conditions. We will then examine differences among the conditions.

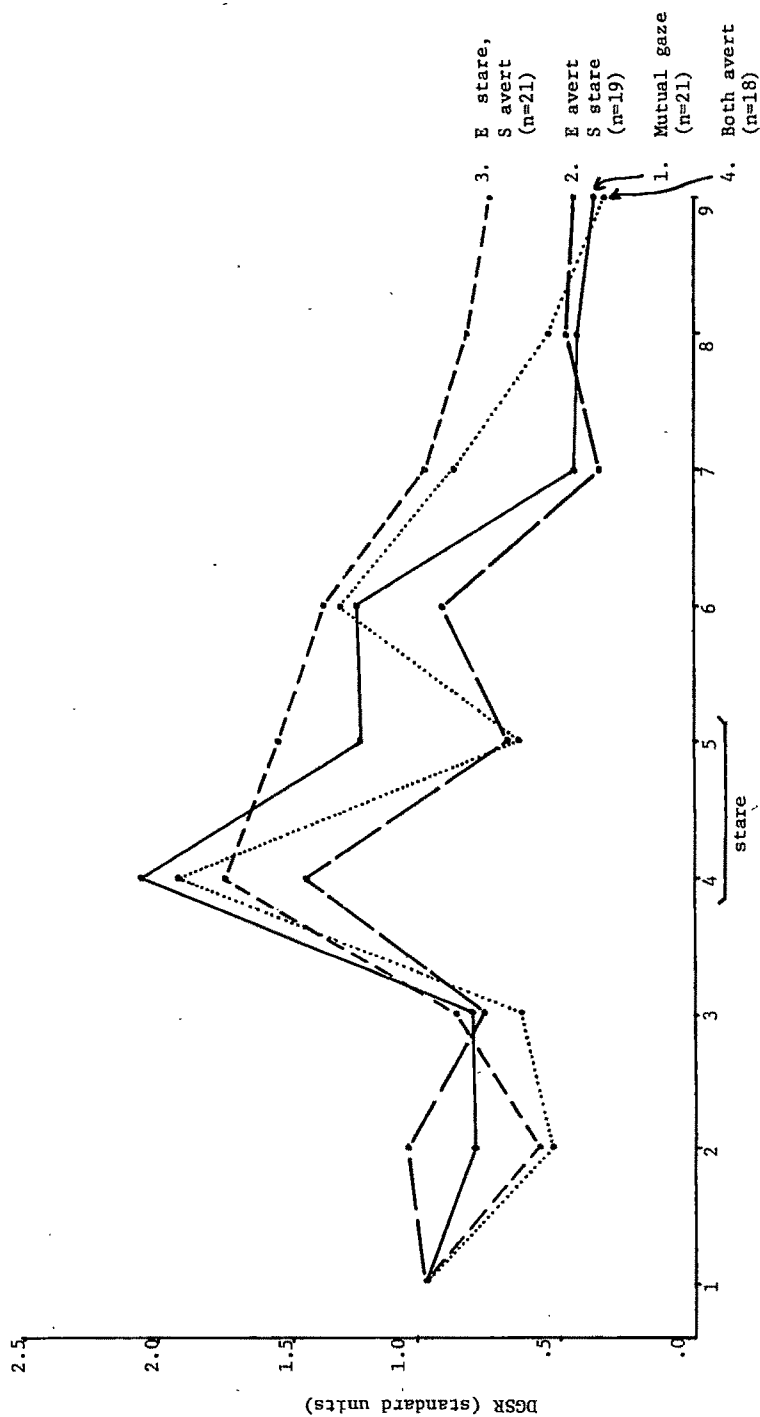
Trends for two variables, DGSR (fig. 2) and pulse rate (fig. 3), show similar patterns in all four conditions. The DGSR always peaks sharply at the beginning of the stare (interval 4), returning to near baseline level after the stare. (For each condition, a *t*-test of the DGSR difference between intervals 3 and 4, for paired observations within subjects, gives $P \leq .03$.) Pulse rate decelerates during the stare period in all conditions, reaching a relative minimum in interval 5, the second half of the stare; it increases briefly after the stare and then drops again. (For each condition, a paired-observations *t*-test of the pulse difference between interval 3 and interval 5 [the period of relatively slow pulse] gives $P \leq .06$.)

In all conditions, TBV trends show a dip from interval 3 to interval 4 (usually extending to interval 5) and a rise after the end of the stare period (fig. 4); however, the dip is deeper in condition 1 (mutual gaze) than in the other conditions. (Our *t*-tests on paired observations within subjects show significant declines between intervals 3 and 4, and between intervals 3 and 5, for conditions 1 and 3 ($P \leq .04$); these dips are insignificant in conditions 2 and 4.)

Thus the physiological variables follow more or less similar patterns across conditions, the main exception at this point being TBV, which shows a deeper dip during mutual gaze than during the nonmutual gaze control conditions.

To compare physiological responses among the four conditions properly, we should control on the large intersubject variation in baseline values which is present. This is conveniently accomplished with a regression equation of the form $V_i = a + bV_2 + dV_3 + eC_1 + fC_2 + gC_3 + \text{residual}$. Here V stands for the physiological variable of concern (TBV, pulse rate, or DGSR), and its subscript refers to the time interval in which that variable is measured. We let $i = 4$ or 5 , the time intervals during which staring occurs. Because V_2 and V_3 are measured prior to the stare, by regressing V_4 (or V_5) on them we control for intersubject variation without obscuring differences among the conditions. The symbols C_1 , C_2 , and C_3 represent dummy variables for conditions 1, 2, and 3 (scored 1 if the subject was run in that condition and 0 otherwise; three dummy variables are sufficient to account for four conditions). The letters a , b , d , e , f , and g are constants.⁴

⁴ Sex of Ss and their paid vs. volunteer status were not significantly related to the physiological variables, though men have slightly but consistently lower pulse rates. Some significant effects were attributable to variation among Es; however, these were neither strong nor consistent in pattern. Inclusion of these variables in the regressions produced essentially the same results that are reported in the text.



TIME INTERVAL NUMBER

FIG. 2.—Experiment 1. Rate of change of GSR (DGSR) in 10-second intervals by condition.

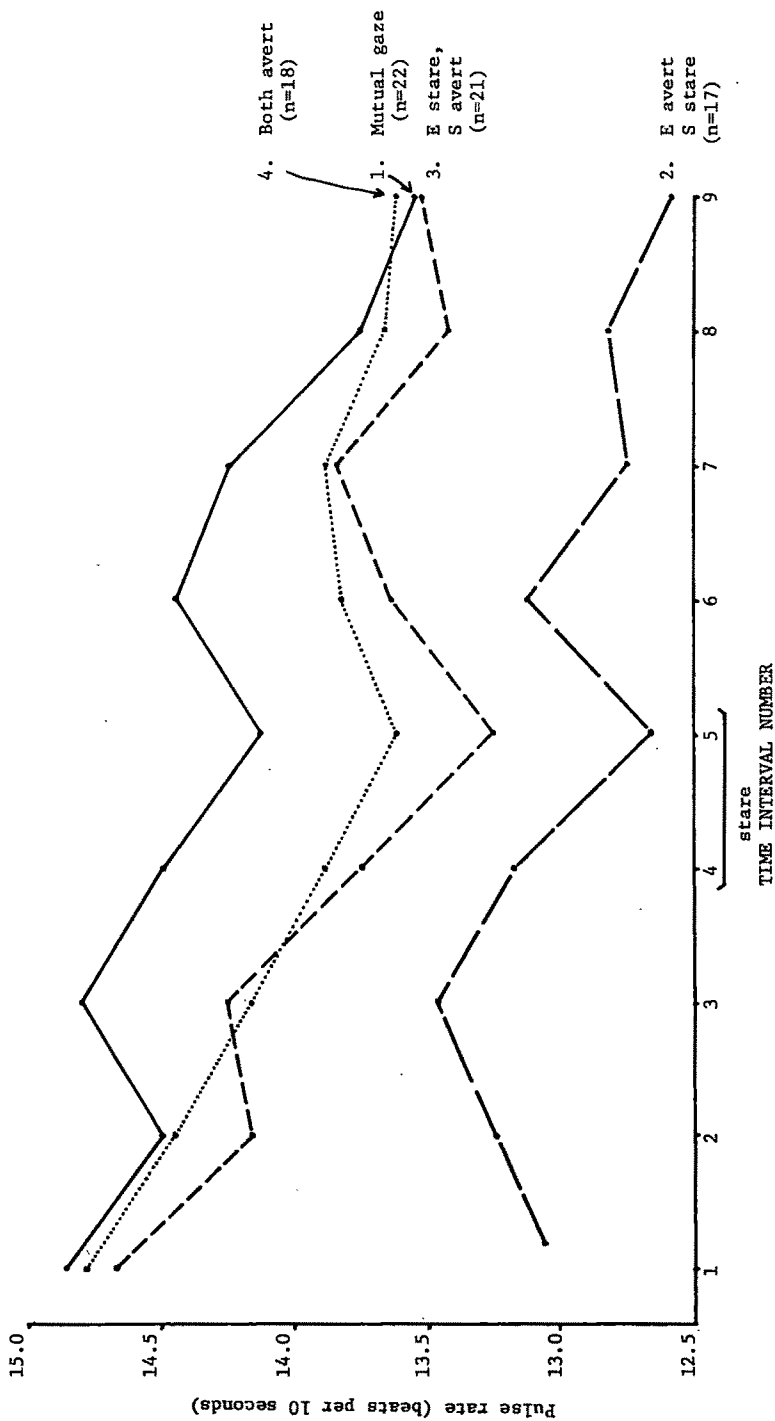


FIG. 3.—Experiment 1. Pulse rate in 10-second intervals by condition.

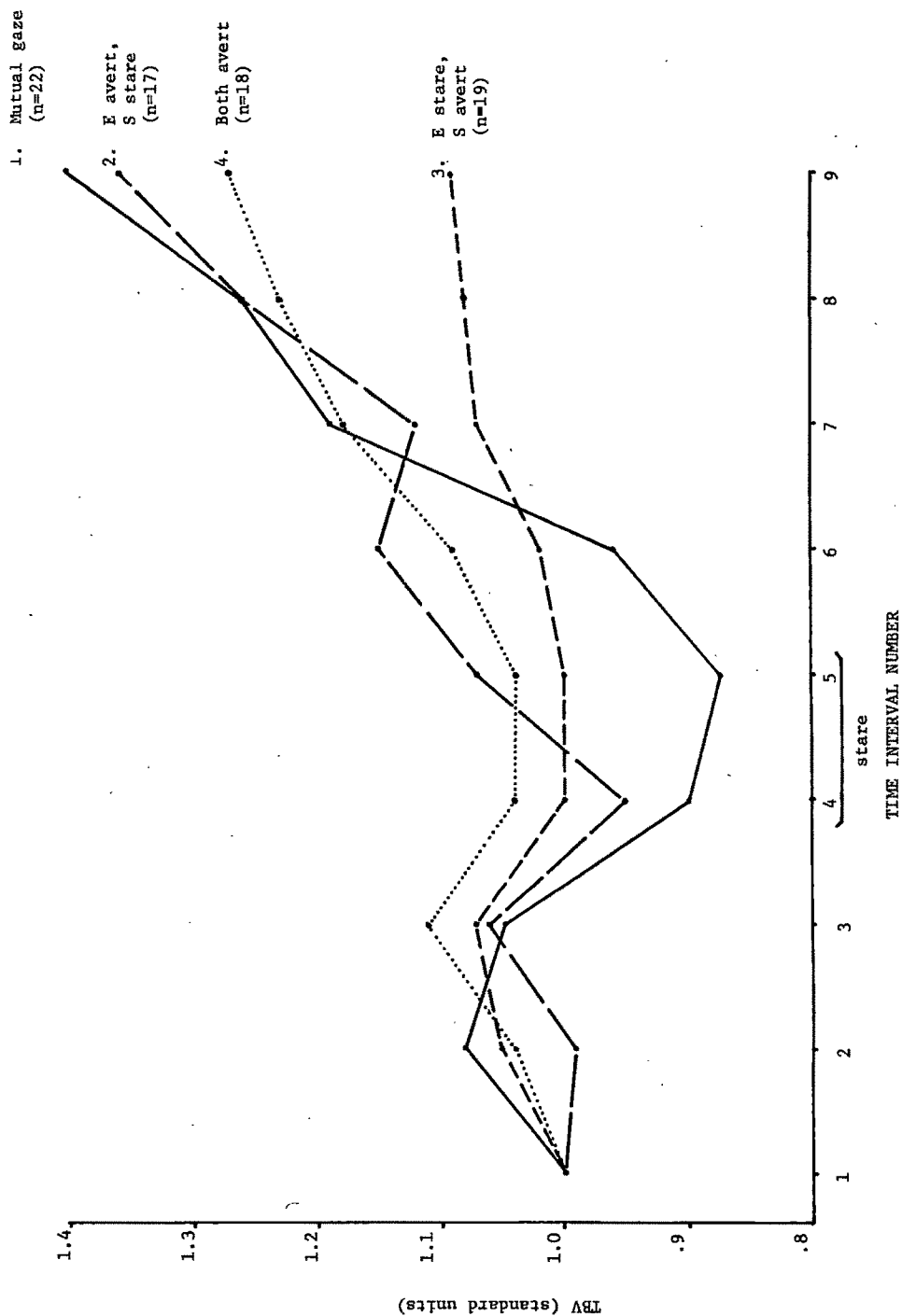


Fig. 4.—Experiment 1. Thumb blood volume (TBV) in 10-second intervals by condition.

Each of the three physiological variables as measured in time interval 4 and each as measured in interval 5, was taken as a dependent variable (i.e., as V_i) for a total of six regression equations. Nonstandardized regression coefficients (with their standard errors in parentheses) are displayed in table 1. Asterisks indicate coefficients at least twice as large as their standard errors, which is approximately equivalent to significance at or beyond the .05 level. In five of the six regressions, V_2 or V_3 or both are significant predictors of V_i , as we would expect given the large intersubject variation commonly found in physiological data.

In equations (1) and (2) the dependent variables are, respectively, TBV as measured in interval 4 and TBV as measured in interval 5. In both of these equations, the coefficient of C_1 is larger than the other dummy coefficients and the only one that is significant. During mutual gaze (condition 1), TBV drops .14-.15 units lower than in condition 4 where E and S stare at inanimate objects. None of the other conditions shows the strength, significance, and consistency of C_1 . Mutual gaze seems clearly different from the other conditions in its ability to lower TBV.

TABLE 1
EXPERIMENT 1. NONSTANDARDIZED REGRESSION COEFFICIENTS FOR
SIX EQUATIONS COMPARING FOUR STARE CONDITIONS

INDEPENDENT VARIABLES	EQUATION NUMBER AND DEPENDENT VARIABLE					
	(1), TBV in Interval 4 ($N=76$)	(2), TBV in Interval 5 ($N=76$)	(3), Pulse Rate Interval 4 ($N=76$)	(4), Pulse Rate Interval 5 ($N=76$)	(5), DGSR in Interval 4 ($N=79$)	(6), DGSR in Interval 5 ($N=79$)
TBV interval 2.....	.24 (.16)	.13 (.17)
TBV interval 3.....	.19 (.10)	.30* (.10)
Pulse rate interval 2.....63* (.10)	.51* (.12)
Pulse rate interval 3.....26* (.10)	.35* (.12)
DGSR interval 2.....	-.49 (.29)	-.18 (.16)
DGSR interval 3.....	1.86* (.33)	1.13* (.18)
C_1	-.14* (.05)	-.15* (.05)	.39 (.31)	.26 (.38)	.02 (.45)	.43 (.25)
C_2	-.06 (.05)	.06 (.06)	.23 (.34)	-.11 (.41)	-.49 (.47)	-.04 (.26)
C_3	-.04 (.05)	-.03 (.05)	.00 (.32)	-.24 (.39)	-.60 (.45)	.61* (.25)
Constant.....	.58	.56	1.16	1.35	.97	.03
R^227	.35	.85	.79	.32	.44

NOTE.—Standard errors shown in parentheses.

* Regression coefficient is at least twice as large as its standard error.

Pulse rates as measured in intervals 4 and 5 are the dependent variables in equations (3) and (4). None of the coefficients of the dummy condition variables approaches significance.

When DGSR is the dependent variable (eqq. [5] and [6]), the only dummy with a significant coefficient is C_3 in interval 5, but this does not fit into a coherent pattern of results so it may be an anomaly.

In sum, the clearest and most consistent difference among the various conditions is the particular ability of mutual gaze to depress TBV. Pulse rate and DGSR do not show systematic differences across conditions.

Discussion.—Our results confirm that mutual gaze is physiologically arousing, but they differ in important detail from earlier reports. We found characteristic patterns of DGSR and pulse rate which occur regardless of whether the stare is mutual, unreciprocated, or directed at an inanimate object; these patterns were *not* associated with mutual gaze per se. However, we did find strong, significant, and consistent drops in TBV during mutual gaze, while this particular response did not occur to any substantial degree during the three control conditions of nonmutual stare. We are confident that the observed patterns of physiological variables are reliable because they occur consistently in our studies reported below as well as in additional experiments which we will not cite in detail here.

The decrease in pulse rate during all stare conditions supports the hypothesis that heart rate decelerates when a subject concentrates on an external stimulus (Lacey and Lacey 1974). The accelerated pulse during mutual gaze, reported by Kleinke and Pohlen (1971), may have been caused by the strangeness of the experimental confederate's behavior rather than the gaze per se, as suggested above. Previous reports of GSR effects from mutual gaze (McBride et al. 1965; Nichols and Champness 1971) did not show strong effects and did not control adequately for habituation, as discussed above. Thus we suggest that these observations were artifacts of experimental design. Our own results are consistent with the widely held view that GSR is a nonspecific response to many types of arousal.

EXPERIMENT 2: EYEBROW SIGNALS

We are less interested in the detailed physiological response to mutual gaze than in the role this response plays in nonverbal communication between actors. From this perspective a primary question is: Can an actor communicate with another person via mutual gaze in such a manner that he manipulates that person's physiology?

Gazes come in many variations. For example, one can glare, gawk, ogle, or leer. The meaning of a gaze can be altered by a tilt of the head, pupil dilation, or widening or narrowing of eyelids to reveal more or less sclera. Eyebrow positions convey a particularly wide range of meanings in con-

junction with a direct gaze. Stares accompanied by lowered brows show anger, aggression, or assertiveness; while raised brows show fear, surprise, disgust, questioning, or retreat (Blurton-Jones and Konner 1971; Brannigan and Humphries 1972; Ekman and Friesen 1975). American respondents judged portrait models to be more dominant when posed with their brows lowered than when posed with brows raised (Keating et al. 1977). We found this eyebrow "signaling" a convenient device to test the hypothesis that an actor can manipulate another person's physiology by variations in gaze behavior. A gaze accompanied by lowered brows should be perceived as more assertive or dominating, and therefore be a greater stressor, than a gaze accompanied by raised brows. Thus we hypothesized that lowered brow gaze would produce a greater physiological effect, specifically a bigger dip in TBV, than raised brow gaze.

Setting.—Same as before except that there were two male Es, both in their thirties, one a graduate student and one a professor. The academic ranks of the Es were not mentioned to Ss.

Subjects.—This time 42 undergraduate Ss (67% male) were recruited as before. About two-thirds of them were run by the professor E while the graduate student ran the others. Subjects were randomly assigned to the "brows-up" ($N = 21$) or "brows-down" ($N = 21$) condition, and both Es had about an equal number of Ss in each condition.

Procedure.—As before, S was seated in chair 2 (fig. 1) and physiological sensors were attached. Toward the end of the five-minute habituation period, E unobtrusively drew a random number to determine S's condition assignment. The time sequence of events following the habituation period was a 20-second baseline, a 10-second instruction period during which E told S to "stare straight at my eyes," a mutual gaze which began when E said "now" and ended 30 seconds later when E said "look away," and an additional 30 seconds of recording after the stare. (Note that the stare lasts 10 seconds longer than in the first experiment.) During the mutual gaze, E raised his eyebrows in the brows-up condition and kept them slightly lowered in the brows-down condition.

Variables.—Same as before.

Results.—Trends for TBV, pulse rate, and DGSR replicate the patterns obtained in the first experiment (fig. 5). (For each condition, within-subjects t -tests show significant dips in TBV between intervals 3 and 4, between intervals 3 and 5, and between intervals 3 and 6 [$P \leq .01$]; significant drops in pulse rate between intervals 3 and 5, and between intervals 3 and 6 [$P = .01$]; and a significant rise in DGSR between intervals 3 and 4 [$P = .01$].) More relevant, of course, are differences between the two conditions.

We control for intersubject variation in baseline values as before with a regression equation of the form $V_i = a + bV_2 + dV_3 + eC + \text{residual}$. The V stands for the physiological variable of concern, and its subscript

refers to the time interval in which that variable is measured. We let $i = 4, 5$, or 6 since these were the time intervals during which mutual gaze occurred. We regress on V_2 and V_3 to remove intersubject variation. The C is the dummy condition variable, scored 1 in the brows-down condition and 0 in the brows-up condition. The letters a, b, d , and e are constants.⁵

Each of the three physiological variables as measured in stare intervals $4, 5$, and 6 was taken as a dependent variable, for a total of nine regression equations. Nonstandardized regression coefficients (and their standard errors in parentheses) are displayed in table 2, where asterisks indicate coefficients which are at least twice as large as their standard errors. The coefficients of C are of particular concern since they indicate the degree of difference between the two conditions.

In equations (1), (2), and (3) TBV is the dependent variable. The coefficients of C are significant in two of these equations and very nearly significant in the third. The coefficients are negative indicating that TBV is 0.08 – 0.11 units lower during mutual gaze with brows down than with brows up.

None of the coefficients of C approaches significance when the dependent variable is pulse rate (eqq. [4], [5], and [6]) or DGSR (eqq. [7], [8], and [9]).

Discussion.—As hypothesized, the mutual gaze produced a significantly deeper dip in Ss' TBV when E had lowered instead of raised brows. (Pulse rate and DGSR did not differentiate the brow conditions, which is consistent with their lack of differentiating power in the first experiment.) We conclude that an actor can indeed manipulate another person's physiology by altering stare behavior during mutual gaze. What social functions, if any, are served by this form of nonverbal communication?

EXPERIMENT 3: DO STARE DOWNS AFFECT SUBSEQUENT INTERACTION?

The finding that mutual gaze can enable one person to manipulate the physiology of another raises an important question: When two strangers engage in mutual gaze, do their reactions predict outcomes of subsequent interaction? To answer this we designed an experiment in which two subjects, strangers to one another, had a mutual gaze—essentially a staredown—followed by a conversation and then a joint decision-making task in which each had the opportunity to influence the other. Since the mutual gaze or staredown is commonly interpreted as a contest of assertiveness or dominance, we wanted to see whether Ss' reactions to the staredown would predict assertive behavior in the conversation and the decision-making task.

This experiment was designed to answer one other question as well. In our previous experiments, one participant in the mutual gaze was E who held

⁵ Additional regressions including dummy variables for sex and experimenter produced essentially the same results. There were no significant sex effects, though as before men had slightly lower pulse rates, but experimenter effects were occasionally significant.

high status (as a professor or research assistant) relative to the undergraduate subjects. Furthermore, the experiments took place in E's laboratory where he controlled the situation. Did E's relatively high status and control produce the TBV dip during mutual gaze? In this new experiment both participants in the mutual gaze are student subjects with equal status and equal amounts of control—or lack of it—in the lab. If the familiar TBV

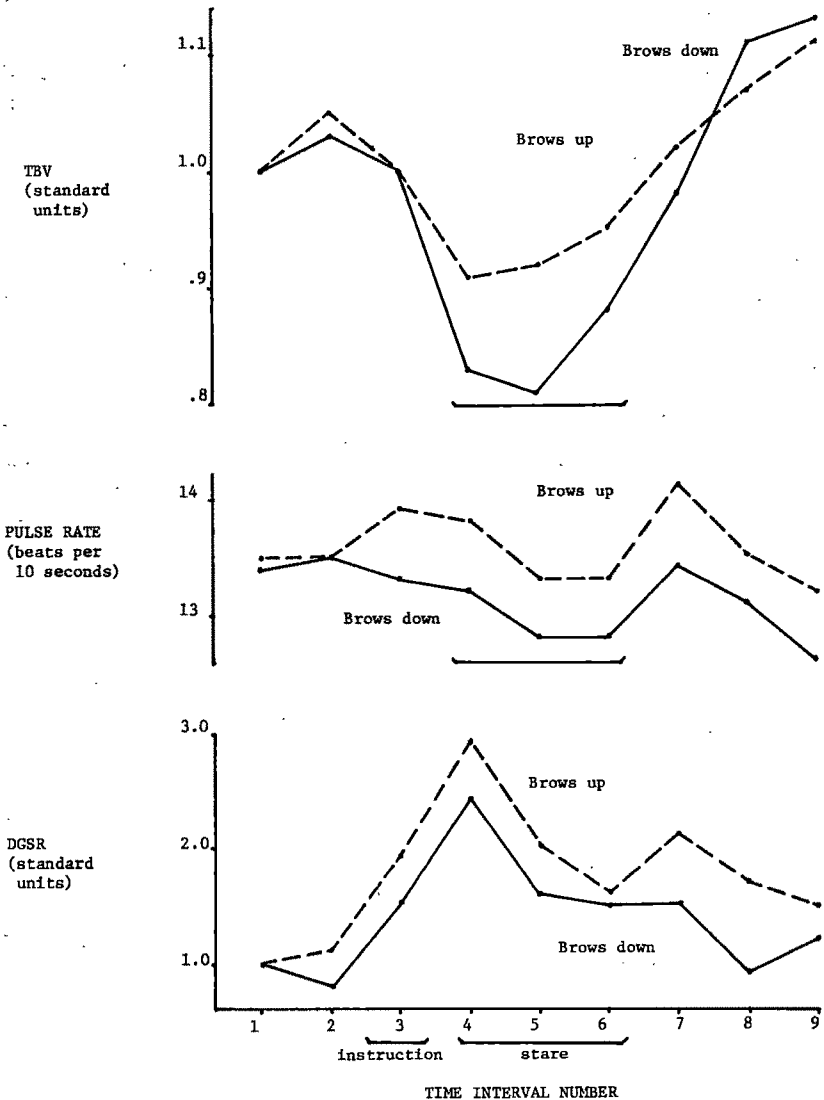


FIG. 5.—Experiment 2. Physiological variables in brows-up ($N = 21$) and brows-down ($N = 21$) conditions.

TABLE 2
EXPERIMENT 2. NONSTANDARDIZED REGRESSION COEFFICIENTS FOR NINE EQUATIONS
COMPARING "BROWS-UP" AND "BROWS-DOWN" CONDITIONS ($N=42$)

INDEPENDENT VARIABLES	EQUATION NUMBER AND DEPENDENT VARIABLE								
	(1), TBV in Interval 4	(2), TBV in Interval 5	(3), TBV in Interval 6	(4), Pulse Rate Interval 4	(5), Pulse Rate Interval 5	(6), Pulse Rate Interval 6	(7), DGSR in Interval 4	(8), DGSR in Interval 5	(9), DGSR in Interval 6
TBV interval 2.....	-.76* (.28)	-.69* (.23)	-.41 (.24)
TBV interval 3.....	.92* (.28)	.74* (.23)	.56* (.24)
Pulse rate interval 2.....42* (.16)	.24 (.18)	.34 (.18)
Pulse rate interval 3.....44* (.16)	.57* (.19)	.47* (.19)
DGSR interval 2.....	-.61 (.57)	.11 (.31)	.05 (.25)
DGSR interval 3.....	1.73* (.26)	.95* (.14)	.59* (.11)
C.....	-.09 (.05)	-.11* (.04)	-.08* (.04)	-.23 (.37)	-.14 (.42)	-.21 (.42)	.17 (.63)	.06 (.35)	.12 (.28)
Constant.....	.78	.90	.82	1.98	2.06	2.15	.22	.79	.45
R ²26	.30	.17	.76	.69	.69	.58	.63	.50

NOTE.—Standard errors shown in parentheses.
* Regression coefficient is at least twice as large as its standard error.

dip appeared here, differences in status and control between E and S could be eliminated as crucial factors.

Subjects.—For this experiment 24 undergraduates (33% male) were recruited through campus ads and paid \$2.00 to participate. They were run two at a time, matched on sex, race, and approximate age and year in school. The Ss in each pair were strangers.

Procedure.—Subjects were greeted on arrival at the lab and kept separated until the experiment began. They were given chair 1 or 2 depending on a random draw. An opaque screen on the table blocked their view of one another. The session was explained as consisting of three separate studies, one on the physiology of staring, one on normal conversation, and one on cooperative decision making. Physiological sensors were attached to both Ss. After a five-minute habituation period E said, "I'm going to remove the screen, but please don't look at each other until I say so. I'm going to start my watch and let some time go by. After a while, I will say the word 'now.' When I do, then stare straight into each other's eyes—try to outstare the other person—until I say 'look away.' Remember, don't look at each other until I say 'now,' and please don't speak or smile." These instructions were intended to define the mutual gaze as a competitive staredown. After 30 seconds to provide a baseline, E said "now," and after a 20-second stare E said "look away." Responses were recorded for another 40 seconds. The screen was replaced on the table, the sensors detached, and Ss were asked to write on a slip of paper a rating of their degree of comfort during the stare. This "comfort scale" ran from 1 to 5 ("1 means you weren't at all uncomfortable and 5 means you were very uncomfortable") and was later dichotomized at the median. The E collected the slips and said, "Now I'm going to remove the screen once more. This time I'd simply like you to have a conversation about anything you want. You might start by introducing yourself. I'll end the conversation in a few minutes by replacing the screen." The conversation was videotaped with unobtrusive cameras. After three minutes E replaced the screen, terminating the conversation. He then turned on a tape recorder which gave instructions for a standard decision-making task described below. After participating in this task, Ss were fully debriefed.

Variables.—Physiological variables were measured as before, and Ss provided self ratings of their degree of comfort during the staredown, as described above.

The measurement of dominance in a conversation is problematic because the rapidly developing literature on face-to-face communication does not show consensus on the best means for evaluating status or leadership. We chose one simple indicator of conversational dominance: The S who spoke first, after E removed the screen to begin the conversation, was scored more dominant. This indicator misses all of the richness of conversational interaction; however, it has important advantages. First, independent coders

could score Ss as more dominant or less so (or tied) from videotapes with virtually perfect reliability. Second, this indicator is strongly correlated to other measures of status (Rosa and Mazur [1979]; also see related studies by Willard and Strodtbeck [1972]; and Koomen and Sagel [1977]), a result which we replicate below.

Our joint decision-making task was the standard influence-measuring procedure developed by Joseph Berger (Berger, Fisek, and Conner 1974; see also Mazur 1975), so evaluation of relative dominance was straightforward. Prerecorded instructions for this task tell Ss that they will be working cooperatively on a decision-making task involving a series of binary choices. They are then shown a series of slides, each consisting of two patterns composed of black-and-white rectangles. They are asked to decide, for each slide, which of the two patterns contains the greatest white area. Actually, the patterns are constructed so that both contain the same area of white, and previous standardization has shown that the actual probability of picking either pattern is about .5. For each binary trial, each S makes an initial decision and enters his choice on the keyboard of an interaction control machine. Then the machine indicates the other S's initial choice and each S is asked to make a final decision. Actually, Ss are given false information about the other person's initial choice. The interaction control machines are rigged to indicate that the Ss agree or disagree on their initial choices according to a prearranged sequence. A "switch" is defined as a disagreement trial in which an S's final choice is different from his initial one. A switch is presumed to indicate that the S has accepted influence from his or her partner. For each pair, the S making the fewest switches was regarded as least influenceable or most independent. We took this as an indicator of relative dominance.

Thus Ss were scored more or less dominant, or tied, in two situations: the conversation and the joint decision-making task. The gamma correlation between these rankings was high (.65), so we combined them into a single three-point index of dominance. Those who were more dominant in both situations (six Ss) were called "high dominant"; those who ranked less dominant in both situations (six Ss) were low; and the remaining 12 Ss with mixed or tied ranks were placed in the middle.⁶

Results.—Our first goal was to see whether response to the mutual gaze predicts dominance in subsequent interactions. None of the physiological variables was significantly related to our index of dominance. However, the subjective experience of the stare, as measured by the comfort scale, was a strong predictor. Those who were most comfortable during the mutual gaze tended to score high on the combined index of dominance in the subsequent interactions (gamma = .73; $P \leq .05$).

⁶ In the debriefing session, only three Ss questioned the veracity of the decision-making tasks. All Ss were included in the analysis.

Our second goal was to see whether mutual gaze between two Ss of equal status and equal (minimal) control of the situation would produce the same patterns of physiological response as obtained when E was one of the participants in the mutual gaze. Patterns for all three variables (TBV, DGSR, and pulse rate) are shown in figure 6, and it is apparent that they are similar to patterns obtained in the earlier experiments. (Within-subjects *t*-tests

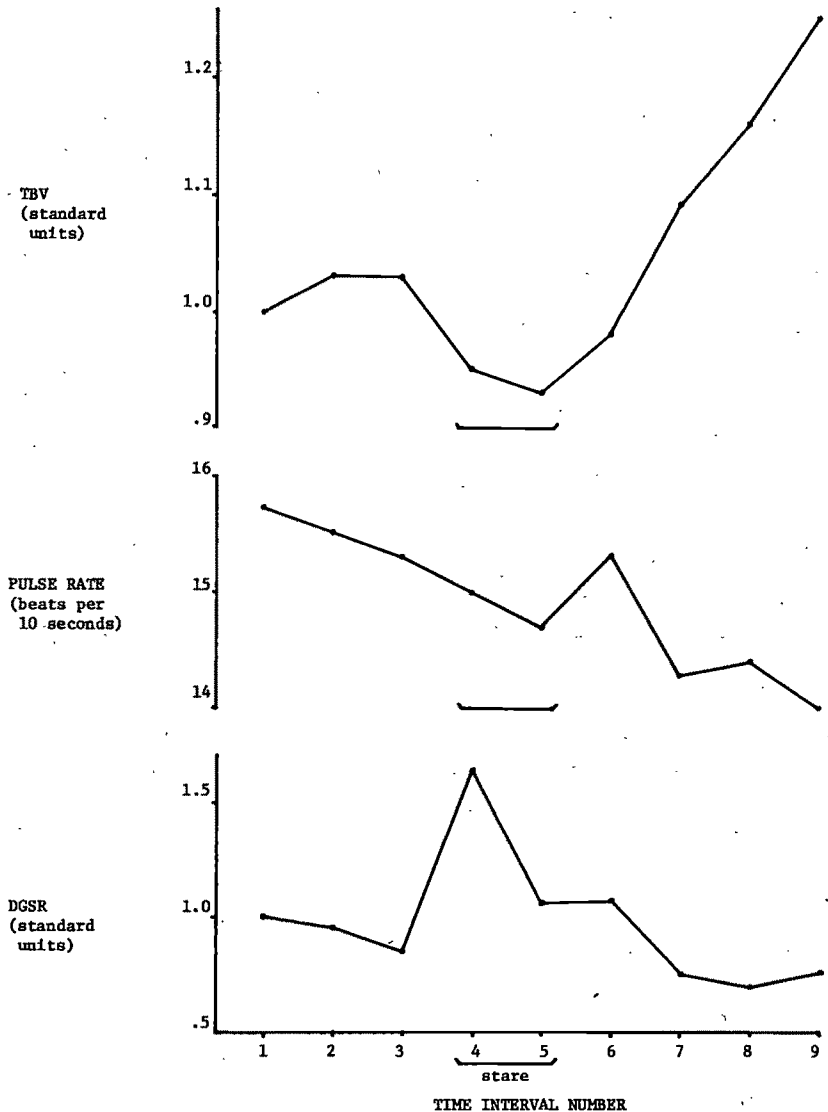


FIG. 6.—Experiment 3. Physiological variables in 10-second intervals for students in mutual gaze.

show significant dips in TBV between intervals 3 and 4, and between intervals 3 and 5 [$P < .05$]; a significant drop in pulse rate between intervals 3 and 5 [$P < .01$]; and a significant rise in DGSR between intervals 3 and 4 [$P < .01$].)⁷

Discussion.—Our finding that Ss' levels of comfort during the staredown predict degree of dominance in subsequent interaction is consistent with two models of status formation. The staredown may serve as a contest in which each person tries to "outstress" the other. The "winner" is the person who subjectively experiences less discomfort during the contest and, having won, he or she is likely to take the dominant role in the conversation and decision-making task, a role which the "loser" accepts. In this interpretation, the staredown is decisive; its outcome affects subsequent relations between the individuals involved. In an alternate model, one of the subjects comes into the experimental situation with an edge—perhaps because of some personality trait, greater experience as a leader or as an experimental subject, better health, more rest the night before, etc. If one subject does have an edge from the beginning, whatever the reason, then that person might "excel" in all phases of the experiment—that is, he would be more comfortable during the staredown, more assertive during the conversation, and more influential during the decision-making task. In this interpretation the staredown is not decisive since the relative status of the participants was predestined from the beginning. Unfortunately we are not able to eliminate either of these models at this point.

The physiological patterns obtained in this experiment are quite similar to those obtained in the mutual gaze conditions of our earlier experiments. This result clearly indicates that these responses are not strong functions of the status difference between the participants in the mutual gaze or of E's control over the laboratory situation. Two additional studies, not cited here in detail, corroborate this result. In one experiment, undergraduate Ss participated in mutual gaze with a male E who was a graduate assistant. Half of the undergraduate Ss were told that E was a professor and the other half were told that E was an undergraduate research assistant; E was blind to the Ss' expectations about him. We found essentially no difference in physiological responses between Ss in these two conditions. In a second experiment, pairs of naive Ss stared at each other, one member of each pair being a student and the other a real professor. We found no significant differences in physiological responses between students and professors. (In both of these experiments, the physiological patterns observed were similar to those reported above.)

⁷ Sex and seat position of Ss were not significantly related to the physiological variables, to ratings of comfort, or to performance on the decision-making task.

OVERVIEW

Most of the physiological patterns we observed were extremely reliable, appearing across all conditions of an experiment as well as being repeated from one experiment to another. At the beginning of each stare DGSR peaks sharply, regardless of whether the stare is directed at another person or an inanimate object. In experiment 2, with a 10-second instruction period preceding the stare, the rise in DGSR begins earlier than in the other experiments (fig. 5), suggesting that E's speech has elicited some of the activity. In sum, DGSR appears to be a very general response affected by a wide variety of stimuli. If mutual gaze has an effect on DGSR over that of nonmutual gaze, the difference is overwhelmed by the larger general arousal response.

We consistently find a drop in pulse rate during the second 10 seconds of the stare. Like DGSR, this response occurs whether the stare is directed at another person or at an inanimate object. In experiment 2 the stare lasted 30 seconds instead of 20 as in the other experiments, and the lowered pulse was maintained throughout the duration of the stare (fig. 5). These results consistently support the Lacey's hypothesis of pulse deceleration in a subject who is attending to an external object, and they also show that there is about a 10-second time delay before the deceleration becomes apparent.

For our purposes the most interesting variable is TBV, the only physiological indicator which differentiates mutual from nonmutual gaze. The clear and consistent dip in TBV which appears in mutual gaze, but not in the control conditions of nonmutual gaze, is an indication that blood is moving from the periphery of the body to the heart, lungs, and large muscles. This is a normal stress reaction which occurs in many situations besides mutual gaze; however, in our context, it demonstrates that mutual gaze is a greater stressor, causing more physiological arousal, than nonmutual gaze.

There is a danger of overinterpreting the extreme reliability of our physiological patterns. All of the experiments were performed in a special social setting: a social psychology laboratory. All of the participants knew they were subjects in an experiment and all were wired with electrical sensors. Reactions which occur in this setting may not be typical of everyday experience. Within this context there were variations in personal characteristics of experimenters, in the relative status and control of the participants in the staring, in sex of subjects, and in details of procedure; and none of these factors altered the basic patterns. However, we do not know what sort of patterns would occur in a very different setting or even in the same setting if the situation had been defined differently. We do know that there was great variation among individual subjects. The TBV dip during mutual gaze is an aggregate effect based on all Ss in that condition; some subjects

showed deep dips and some no dips at all when involved in mutual gaze, and we cannot explain this variation.

In general, Es were able to manipulate TBV of Ss through simple movements of the eyebrows: lowered brows produced significantly deeper TBV dips than raised brows. This is probably explained by the tendency of Americans to perceive a person as more dominant with lowered brows than with raised ones (Keating et al. 1977). Presumably, other modifications of the stare could produce the same effect. The essential point is that an actor can manipulate elements of the stress response in another person through nonverbal signals in the eye contact channel.

Subjects' responses to a staredown were strong predictors of their degree of dominance in later interaction. Those who were more comfortable during the stare were more likely to be assertive in a subsequent conversation and to be influential in the following decision-making task. Perhaps the staredown served as a nonverbal contest to determine who would dominate the dyad. Or perhaps a dominance-deference relationship was predetermined before the pair entered the laboratory and was simply played out in the staredown as well as in the conversation and the decision task. At present, we cannot eliminate either of these models. However, both models make the important point that indicators of status differentiation are apparent from the earliest moments of interaction between strangers. While we do not know whether this early differentiation remains stable over longer time periods, it does suggest a departure from traditional theories which picture the emergence of status differences as a relatively long-term process in which the members of the group remain undifferentiated until they have an opportunity to evaluate each other's contribution and then award high (or low) status in recognition of each member's worth (Homans 1961; Blau 1964). Our results are consistent with empirical work by Fisek and Ofshe (1970) who formed three-man Bales-type groups using students who were initially undifferentiated on obvious status characteristics such as sex, race, and age. About half of their groups formed a stable status order within the first minute of interaction. More recently, Rosa and Mazur (1979) were able to make moderately good predictions of students' rankings in similar Bales-type groups by observing their eye contact behaviors during the first 15 seconds of interaction. These results seem to contradict the traditional view of long-term status emergence.

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Considering Divorce: An Expansion of Becker's Theory of Marital Instability¹

Joan Huber

University of Illinois at Urbana-Champaign

Glenna Spitze

State University of New York at Albany

Using a national probability sample ($N = 1,360$) of husbands and wives married to one another in 1978, we explore the correlates of thinking about divorce in order to extend Becker's theory of marital instability by adding sociological variables and measuring individual utilities. Wives' thoughts of divorce increase with their work experience, having a youngest child aged 6–11, and egalitarian housework attitudes and decrease with age at marriage, marital duration, and husband's housework contribution. Husbands' thoughts of divorce increase with wife's work experience and wife's egalitarian housework attitudes and decrease with the presence of children under 6, marital duration, and age differences. To the extent that thought of divorce relates to eventual divorce, these findings imply that the husband's earnings and the presence of children may deter divorce less now than they have in the past.

The purpose of this paper is to test the effect of individual self-interest on reported thoughts about divorce. In preindustrial society, marriage and reproduction tended to be governed by unconscious rationality, collective behavior patterns that benefit a species despite individual unawareness of them (Wrigley 1978, p. 135). Industrialization shifted the motivation for much individual behavior from collective folk wisdom to conscious self-interest. The theory of the demographic transition describes how reproductive behavior responded to increasing costs and decreasing rewards for childbearing.

In line with the popular belief that couples should marry for love, not to please their parents, individual self-interest increasingly dominated 20th-century marriage patterns. The decline in family functions led sociologists to conclude that personal relationships were the family's most important social contribution (Ogburn 1933, p. 692). Beginning in the 1930s, social

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scientists vigorously (but inconclusively) sought the secret of marital adjustment (Kirkpatrick 1968), but the search for a satisfactory theory of marriage met limited success. In the 1950s, a popular theory held that marriage fills complementary instrumental and expressive needs. By the 1970s, this theory was discredited; women's contribution to subsistence and men's contribution to expressive activities have been much greater than earlier theorists had supposed (Aldous 1977, p. 177). Empirical studies of the correlates of marital happiness also met difficulties. A recent sophisticated study reports that the lack of strong positive associations between reported marital happiness and a number of status variables unexpectedly casts doubt on a number of widely held generalizations about marriage (Glenn and Weaver 1978, p. 276). Yet the accelerating rise in divorce (Westoff 1978) indicates a need for a theory of marital stability.

A theoretical approach that may be fruitful because it includes factors that are historically associated with the rise in divorce is Gary Becker's (1973, 1974, 1976) economic theory of marriage, based on individual-level empirical findings and recently extended as a theory of marital instability (Becker, Landes, and Michael 1977). According to Becker, marriages dissolve when the utility expected from staying married falls below the utility expected from divorce. The theory assumes that individuals maximize utilities from commodities they expect to consume in a lifetime, ranking marital strategies by their full wealth and choosing the highest. Since uncertainty prevails, unfavorable outcomes may occur.

Becker predicts that dissolution probabilities are lowered by an increase in the expected value of positively sorted variables (e.g., men's earnings); an increase in the time spent seeking a spouse (age at marriage); and an increase in marital-specific capital (e.g., children), which increases with duration and decreases with order of marriage. Dissolution probabilities are raised by unexpected changes in values of positively sorted variables (e.g., husband's earnings), by an increase in the expected value of negatively sorted variables (e.g., wife's earnings relative to husband's), and by a larger discrepancy in mate traits (e.g., IQ, religion, race) than would occur in optimal sorting (Becker et al. 1977, pp. 1156-57).

Becker's predictions are generally consistent with empirical findings. Husband's income relates negatively to marital dissolution (Cutright 1971; Becker et al. 1977). But recent research on the distinctive effects of husband's employment stability and income level—a prominent topic during the Depression—shows that husband's employment stability decreases the probability of dissolution regardless of income level (Cherlin 1979; see also Ross and Sawhill 1975). An increase in wife's income and the ratio of her earnings to family income increases dissolution (Ross and Sawhill 1975; Cherlin 1976, 1978; Waite and Moore 1978). Age at marriage (Bumpass and Sweet 1972; Ross and Sawhill 1975; Becker et al. 1977; Waite and Moore

1978) and duration of marriage relate negatively to dissolution (Ross and Sawhill 1975; Cherlin 1977), although duration does not predict whether couples who apply for divorce will carry through or dismiss the action (Levinger 1979, p. 148). Marital-specific capital deters dissolution when it takes the form of assets (Cherlin 1977; Waite and Moore 1978), but the effect of young children is unclear (Becker et al. 1977; Cherlin 1977; Waite and Moore 1978). Discrepancies in spouse traits such as age and religion increase dissolution (Bumpass and Sweet 1972; Becker et al. 1977). Finally, marriages beyond the first are slightly more likely to dissolve (Becker et al. 1977; McCarthy 1978).

While Becker's theory explains a variety of findings, it has several problems from a sociological perspective. First, it fails to predict explicitly the utility of situations for the individuals experiencing them. Economists take utility as a given because their deductive theories cannot readily handle inexact variables (Maynes 1978, p. 391), hence their utilities tend to be commonsense notions about typical human preferences. Yet one cannot know why spouses decide to divorce unless one knows how they evaluate a particular situation.

Second, every divorce involves two people whose experience and perceptions of benefits and costs may differ. In principle, Becker's theory could separate such effects, but in practice Becker makes no attempt to do so. Yet some variables could oppositely affect husband's or wife's desire for divorce. A wife's high earnings may motivate her to end a bad marriage; this effect could be reinforced if the husband were threatened by his wife's earnings (Komarovsky 1973) or if he felt that her earnings relieved him of financial responsibility. In contrast, the wife might be a more attractive partner if she had higher earnings; her motivation to end the marriage might be offset by his desire to continue it. In sum, Becker's theory predicts that a particular event will increase the probability of dissolution, but it does not tell us why.²

Third, because Becker's theory does not distinguish the ways men and women may perceive costs and benefits, it cannot take into account new evaluations of the household division of labor. Although wives' employment has become acceptable because husbands like living in two-earner families, the division of household labor is now more likely to be contentious than it was in the past. Married women, fully employed or not, typically perform most of the daily care of house and children (Farkas 1976; Walker and Woods 1976). At the turn of the century, the typical husband was a manual worker or farmer. Today a majority of husbands work in white-collar jobs,

²In fact, little is known about the extent to which divorces are desired by husbands, wives, or both, and about what factors stimulate desire for dissolution (Glick 1977, p. 101). A recent study of clergy, psychological counselors, and lawyers suggests that nonmutuality in the decision to divorce is the rule rather than the exception (Kressel et al. 1979, p. 256).

labor that is in striking contrast to the work wives do at home. Hence women's household duties may seem more onerous now. Wives who reject traditional role definitions may evaluate their marriages according to whether they see their husbands doing a fair share of the daily housework. Indeed, sex-role attitudes may be so basic in marital interaction that major differences may influence both spouses to think about divorce more often.

Although this study tests hypotheses derived from Becker's theory, modified to reflect the individual motivations of husbands and wives, and expands the theory by assessing the effect of the household division of labor and of differences in husband/wife sex-role attitudes, we cannot directly test Becker's theory itself because we use a different dependent variable. Using data from our national probability sample designed to study the social bases of sex-role ideologies, we wanted to study the effect on husbands and wives of factors that have only recently varied enough to be useful as variables: sex-role attitudes and the household division of labor. However, these new variables appear in no longitudinal study of divorce, and our cross-sectional study cannot test causes of divorce. Hence we use the respondent's thought of divorce as a dependent variable, and we assess which background factors are associated with it. Since the relation of thought of divorce to divorce itself is unknown, we present the following rationale for our choice.

Our dependent variable is the question used by Campbell et al. (1976, p. 322): "Has the thought of getting a divorce from your husband/wife ever crossed your mind?" Choosing this wording in order to make it easy for respondents to indicate even ephemeral doubts, Campbell expected that many respondents would admit to having "ever" thought about divorce. Sociologists with whom we discussed the item were also sure that the thought of divorce must have crossed almost everyone's mind. But this expectation is wrong; about 64% of the women and 71% of the men in the Campbell study reported that they had never thought of divorce, and the response correlated highly with degree of satisfaction with the marriage and with whether the respondent had ever wished to be married to someone else (Campbell et al. 1976, p. 322).

Logically, marital satisfaction, thought of divorce, and divorce itself lie on a continuum, with fewer persons in successive categories. The propensity to end a bad marriage intervenes between marital satisfaction and divorce. Unhappily married people think of divorce only if it is possible for them, and presumably only a fraction of those who consider divorce translate thought to deed. Our measure separates spouses' motivations and sorts out persons who are not only dissatisfied in marriage but also willing to consider divorce; it should be quite stable since it concerns having "ever" thought of divorce. Measures of marital happiness are subject to short-term fluctuation (Glenn and Weaver 1978, p. 275). Perhaps the best reason for exploring the corre-

lates of thinking about divorce is the need to know much more about the new realities of family life, especially because of their potential effects on children (Bumpass and Rindfuss 1979, p. 64).

HYPOTHESES

Because the most critical factors historically associated with the rise in divorce are the fertility decline and the increase in women's labor force participation, we expect that the wife's work history and earnings, the presence of children, and the wife's definition of a fair division of household labor will most importantly affect thought of divorce. Specifically, we expect that:

1. Wives are more likely than husbands to think of divorce and to think of it more often because they are more likely to define the domestic division of labor as unfair.

2. We modify Becker's hypothesis that husband's earnings decrease the probability of divorce because (1) it may be time bound, better suited to a period when most wives of high-income husbands were not employed, making divorce costly for husbands and especially for wives; and (2) at low-income levels, husband's employment stability is more important than his income level (Ross and Sawhill 1975; Cherlin 1979). Since we lack data on husband's work history, we hypothesize only that husband's income will insignificantly affect thought of divorce for both spouses.

In contrast, we expect the ratio of wife's earnings to husband's and also the wife's work history—her employment stability—to affect both spouses' thought of divorce. The effects should be stronger for wives because the impact of their labor force participation and earnings is direct, hence presumably stronger than it is for their husbands.

3. Persons who marry young are more likely than others to experience divorce because of the limited time invested in search. This should also affect thought of divorce for both spouses, but the effect may be greater for the husband since his ability to acquire information about his qualities relative to those of other potential spouses is less limited by early marriage than is the wife's, whose activity may be constrained by early pregnancy.

4. An increase in such marital-specific capital as young children should reduce the probability of thinking about divorce for both spouses, but more for the wife than for the husband, since she typically, if divorced, must care for the children and work outside the home.

5. A larger discrepancy in spouse traits (education, age, religion) should raise the probability of thinking of divorce for both spouses. Divorce may be contemplated more often by the spouse with the higher level of a trait that can be quantitatively evaluated (education).

6. Thought of divorce will decline with marital duration, owing to the

acquisition of marital-specific capital. We expect the effect to be greater for wives than for husbands because wives' value in the marriage market declines much more sharply with age. Also, women's investments in household or market skills appreciate much less over time than do men's market skills; household skills reach maximum potential quickly, while women's market skills may depreciate or appreciate slowly because of discontinuous labor force participation and job choices made for family convenience.

7. Probability of thought of divorce is higher in marriages beyond the first, both because divorce-prone persons should be found in higher-order marriages and because such marriages accumulate less marital-specific capital (such as children) than first ones. If the major cause of this higher probability is "divorce proneness," thoughts of divorce should increase for previously divorced persons but not for their new spouses. If it is marital-specific capital, both a previously divorced person and a spouse in a second marriage should think of divorce more often. We test these alternatives by determining whether neither spouse, both, or only the previously divorced one is affected.

8. Differences in husband's and wife's attitudes on women's proper roles should increase thoughts of divorce for both spouses, but especially for wives.

9. Employed wives who feel that husbands should equally share housework but who also do most of the housework themselves will think of divorce more often than will other wives.

DATA AND METHODS

This study is based on telephone interview data obtained in October and November 1978, from 680 married couples ($N = 1,360$), part of a national probability sample of U.S. households. Following Waksberg's (1978) variant of Sudman's (1973) random digit dialing procedures, telephone exchanges are randomly chosen, then random numbers are dialed within each exchange so that both unlisted and listed numbers are included.

Our main dependent variable is a dichotomy, coded 1 for yes and 0 for no, in response to the question, "Has the thought of getting a divorce from your husband/wife ever crossed your mind?" Using regression with splits near or outside the 25%-75% range may bias findings because it violates several assumptions of regression analysis (Gillespie 1977). We therefore use probit analysis, a maximum-likelihood technique suited to estimate a dichotomous dependent variable. We report probit slopes evaluated at the same point on the curve ($P = .30$) for husband and wife equations. These slopes can be compared across equations and interpreted similarly to unstandardized regression coefficients.

Our independent variables are in four categories. First, individual charac-

teristics are measured for husband, wife, or both: age at marriage, coded in years; income, coded in dollars per year; previously divorced, coded 1 = yes, 0 = no. Two characteristics of the wife's employment include current employment, coded 1 = current full-time or part-time employment and 0 = no employment. The wife's work history is measured by an index which includes number of years worked in the past 10, adjusted for part-time and part-year work.

Second, objective characteristics of the marriage include duration in years and age of youngest child (coded as two dummy variables, one for 0–5 years, another for 6–11 years). Spouse's age and education differences are computed by subtracting the wife's years from the husband's. Religious difference is coded as a dummy variable, 1 = difference, 0 = both Protestant, Catholic, Jewish, other, or no religion. We also included wife's earnings as a percentage of family income.

Third, subjective characteristics of the marriage include both spouses' perceptions of who prepares meals, shops for food, cares for children or old people, does daily housework, and cleans up after meals—the five tasks that comprise the major time spent in housework (Walker and Woods 1976). An index is based on the number of these tasks for which the husband does half or more of the work. We also include spouses' responses to the question, "If a husband and wife both work full time, do you think that the wife should be responsible for the daily housework [coded 1], that the husband should help her [coded 2], or that the husband and wife should share daily housework equally [coded 3]?"

Fourth, two questions measure spouses' sex-role attitudes: "A married woman should be able to have a job even if it is not always convenient for her family" and "By nature, women are happiest when they are making a home and caring for children." These two items were recoded so that 4 indicated the most liberal response and 1 the most traditional. Responses were summed, then we constructed a difference score between husband and wife.

Several variables were eliminated after preliminary runs revealed problems of multicollinearity. We could not include both husband's and wife's earnings and wife's percentage of family income. Since absolute earnings were less important theoretically (and also had no effect), they were omitted from the equations. Also, we could not include wife's work history and current employment status in the same equations. Since current work status could be a response to as well as a cause of thoughts of divorce, it was omitted in favor of the more straightforward work history measure.

FINDINGS

1. As expected, more wives (30%) than husbands (22%) had ever thought about divorce and they had thought of it more often. Fifty-one percent of

the wives of husbands who said yes also said yes; 38% of the husbands of wives who said yes also said yes. Of wives who ever thought of divorce, 8% had done so often; 11%, sometimes; 21%, once in a while; and 60%, hardly ever; of husbands, 6%, 8%, 16%, and 70% (see table 1 for means and standard deviations of all variables).

2. As predicted (in contrast to Becker), the absolute level of the husband's earnings had no effect on either spouse's thoughts of divorce, nor did the absolute level of the wife's earnings. We expected wife's earnings as a percentage of family income to affect wives' more than husbands' thoughts of divorce, but it affected neither spouse (see table 2). Although the wife/husband earnings ratio may affect the decision to dissolve a marriage, it apparently fails to affect initial thoughts of divorce.

We also tested the effect of the wife's history of employment in the past 10 years. The number of years she was employed (adjusted for part-time and part-year work) positively affected thought of divorce for both spouses,

TABLE 1
MEANS, STANDARD DEVIATIONS, AND VARIABLE DESCRIPTIONS

Variable	Mean	SD	Description
Husband's divorce thought...	.23	.46	Has the thought of getting a divorce from your husband/wife ever crossed your mind; 1 = yes, 0 = no
Wife's divorce thought.....	.30	.42	
Wife's earnings/family income.	.28	.18	Wife's earnings as a percentage of total family income
Wife work past 10 years.....	4.13	3.77	Adjusted (part year, part time) number of years wife worked in past 10
Husband's age at marriage....	25.20	6.79	Husband's age at marriage in years
Wife's age at marriage.....	22.55	5.64	Wife's age at marriage in years
Youngest child under 6.....	.25	.43	Youngest child less than 6; dummy variable
Youngest child 6-11.....	.21	.40	Youngest child 6-11; dummy variable
Education difference.....	.40	2.39	Years difference in husband/wife education level
Age difference.....	2.66	4.58	Years difference in husband/wife age
Religious difference.....	.22	.41	Husband/wife in different categories: Protestant, Catholic, Jewish, other, none, dummy variable
Marital duration.....	15.20	11.43	Years duration, present marriage
Husband previous divorce....	.13	.34	Husband divorced at least once; dummy variable
Wife previous divorce.....	.13	.33	Wife divorced at least once; dummy variable
Sex-role attitude difference....	.07	.57	-1 = husband more liberal than wife, 0 = both about the same, 1 = wife more liberal
Division of housework.....	.98	1.24	Number of household tasks (preparing meals, food shopping, child care, daily housework, meal cleanup) for which husband does equal share or more
Husband's attitude to housework	.77	.42	Husband/wife believe that housework should be shared equally if spouses work full time; 1 = yes, 0 = no
Wife's attitude to housework..	.78	.42	

TABLE 2

PROBIT SLOPES (Evaluated at $P = .30$) TO PREDICT WHETHER RESPONDENT HAS EVER THOUGHT ABOUT DIVORCE FROM CURRENT SPOUSE ($N = 680$)

Variable	Wives	t-Value	Husbands	t-Value
Wife's earnings/family income...	-.111	-1.10	-.120	-1.14
Wife work past 10 years.....	.011*	-1.99	.010**	1.74
Husband's age at marriage.....	0	-.03
Wife's age at marriage.....	-.010*	-2.31
Youngest child under 6.....	-.032	-.77	-.088*	-1.97
Youngest child 6-11.....	.123*	2.69	.041	.84
Education difference.....	-.001	-.17	.012	1.43
Age difference.....	.004	1.06	-.011*	-2.39
Religious difference.....	.041	.93	.098*	2.16
Marital duration.....	-.004*	-2.20	-.006*	-2.89
Husband previous divorce.....	.003	.58	.004	.07
Wife previous divorce.....	-.023	-.35	-.124**	-1.76
Sex-role attitude difference.....	.036	1.15	.024	.72
Division of housework.....	-.028**	-1.82	-.025	-1.56
Husband's attitude to housework.	-.027	-.64	-.062	-1.38
Wife's attitude to housework.....	.099*	2.18	.075	1.57
Variance explained.....	.0909	...

* $P < .05$.

** $.05 < P < .10$.

about one percentage point for each year of employment. Apparently the wife's potential for economic independence affects both spouses. If the wife worked a substantial portion of the past 10 years, even if she is not currently employed and therefore contributing nothing to the family income, she can find a job more easily and can perhaps support herself. One cannot, of course, deduce spouses' reasoning from these data. Possibly the wife's ability to support herself, thus relieving the husband of the prospect of alimony, is decisive. On the other hand, her employment may threaten her husband, causing marital conflict. Thus, while we found no support for the Becker hypothesis on husband's or wife's earnings, we found that similar reasoning may be applied to the effect of wife's work experience.

3. Age at marriage negatively affected wives but not, unexpectedly, husbands. For every additional year of age at marriage, the wife is 1% less likely to have thought of divorce. Women who marry young have spent less time searching for a spouse and are more likely to have married to cover a pregnancy. We expected age at marriage to affect the husband since he can more easily continue to gather information about alternative mates. Possibly the earlier a woman marries, the more likely are her sex-role attitudes to diverge from her husband's over time.

4. The effect of the age of the youngest child is partly as expected. The presence of a child under six years has a substantial negative effect (9%) on the husband's thought of divorce. This makes sense: when a marriage dissolves, young children typically live with the mother; a husband who does not want to be separated from a young child would tend not to contemplate

divorce during this period. Divorced women are not usually separated from their children, hence they bear a double burden of market work and child care. The positive impact on the wife of having a youngest child 6–11 years old may reflect a tendency for this burden to decrease when children reach school age. A woman who had postponed evaluating her marriage prior to this time may begin to think of divorce when single parenthood appears more feasible.

5. A discrepancy in spouses' education affects neither spouse. Age differences affect only husbands, who think less about divorce if they are older than their wives. Religious differences greatly increase (by 9.8%) the husband's thought of divorce, especially if he is a Protestant married to a wife with no religion or to a Catholic (see table 3). Why should religious differences affect husbands but not wives? Perhaps such husbands resent a wife's influence on the children or her lack of participation in the husband's religious network. For wives it is more normative to tolerate a husband's individual participation in his own network.

6. As expected, marital duration decreases thought of divorce for both spouses, but, unexpectedly, men's thought of divorce decreased about as much as women's, 0.5% for each year of marriage. Wives' special investment in housewife or hostess skills is not significant. Perhaps the thought of losing a longstanding spouse-specific social network—and the prospect of forming another—strikes husbands and wives alike as being just too much work.

7. Like Becker, we expected thought of divorce to increase in marriages beyond the first, but it did not. About 13% of both husbands and wives had experienced previous divorces, but this failed to affect the previously divorced person's or the spouse's thought of divorce. Thus our findings support neither the "divorce-prone person" nor the "marital-specific capital" explanations of the effect of an earlier divorce. However, Becker's (1976,

TABLE 3
THINKING ABOUT DIVORCE, BY RELIGIOUS COMBINATIONS*

Husband's Religion	Wife's Religion	Percentage Husbands Thinking of Divorce	Percentage Wives Thinking of Divorce	N
Protestant.....	Protestant	.19	.30	366
None.....	Protestant	.23	.42	43
Protestant.....	Catholic	.35	.40	20
Catholic.....	Catholic	.24	.28	117
Jew.....	Jew	.20	.25	20
Protestant.....	None	.38	.42	26
None.....	None	.24	.31	26
Total.....23	.30	618

* Combinations with N < 20 are omitted.

p. 1179) empirical findings are mixed; when the large 1967 sample for the Survey of Economic Opportunity was appropriately standardized for age and age at current marriage, the findings did not support Becker's prediction that the duration of marriages is shorter in higher-order marriages. Perhaps many people find the experience of divorce so painful that, even if they have traits—such as genius—that make a good mate hard to find, they avoid thinking of divorce.

8. Unexpectedly, spouse differences in sex-role attitudes had no effect on thought of divorce.

9. We expected that employed wives with nontraditional sex-role attitudes who also saw the domestic division of labor as unfair would be more likely to think of divorce than other wives would, but no such interaction effects (two-way or three-way) occurred (not shown). Yet the test of this hypothesis revealed several interesting findings. Both the wife's perception of the actual division of labor and her attitudes about a fair division for a two-earner family affect her thought of divorce. For each of the five daily household tasks which the husband performs at least half the time, the wife is about 3% less likely to have thought of divorce. Also, if the wife believes that housework should be divided equally in families with two full-time workers, she is 10% more likely to have thought of divorce and her husband is almost (8%) as likely (but not significantly so) to have thought of it. Since about 80% of both women and men hold this view, our findings may simply reflect the extreme conservatism of the other 20%, who believe in a rigid division of household labor and in the sanctity of marriage. But neither spouse is affected by the husband's perception of the division of housework or by his attitudes about it. Since husbands rarely perform more than half the housework, the husband's life may be relatively unaffected by existing variations in the division of labor. In contrast, wives may be more aware of minor differences and consider themselves lucky if the husband performs even one daily task, such as child care or mealtime cleanup.

CONCLUSIONS

Using data from a 1978 national probability sample of husbands and wives married to one another ($N = 1,360$), we have attempted to extend the Becker et al. (1977) theory of marital instability by examining the possibility that spouses' perceptions of costs and benefits may differ, adding to the Becker variables spouses' sex-role attitude differences and their definitions of a fair division of household labor. Before drawing conclusions, we compare our findings with those of Becker et al. (1977) in order to show the extent to which economic factors also influence thoughts of divorce. The comparisons must be interpreted cautiously because (1) we do not know precisely how thought of divorce relates to divorce, and (2) attitudes on

marriage and divorce are changing so rapidly that comparisons at different times may be misleading.

Becker et al. (1977) predicted a negative effect on divorce for the husband's earnings, a positive effect for the wife's; we found no effects for the earnings of either spouse. However, following Becker's reasoning, we found that the wife's work history positively affected thoughts of divorce for both spouses. Becker predicted a negative effect for age at marriage; we found a negative effect for wives but no effect for husbands. Becker predicted that the presence of children would negatively affect the probability of divorce. The presence of a child under six years negatively affected the husband's thought of divorce but there were no negative effects for wives. Instead, the presence of a child aged 6-11 positively affected wives' thoughts of divorce. Becker predicted that spouse trait differences would positively affect divorce; we found no effects for education, age, or religious differences for wives. For husbands, only religion had the predicted effect. Becker predicted that marital duration would negatively affect divorce; we found such a negative effect.

Tests of hypotheses new to this research show that women think more about divorce than men do, and that certain sex-role attitudes fail to affect thought of divorce. Yet the wife's perception of the division of labor for employed spouses does affect her thought of divorce.

Our findings suggest, first, the possibility that in the future a husband's high income will not have as important an affect on marital stability as it has in the past.³ Should our finding also appear in studies of divorce, it could affect the rate of normative change of customs concerning divorce and child custody. In our view the slow development of new norms in this area results partly from the fact that divorce has been relatively uncommon, historically, among groups with the most resources and influence to affect social patterns.

Historically, the husband's income has been critical to divorce or separation for several reasons. The higher the husband's income, the less likely was the wife to be employed, a result of the vast expansion of the middle classes during industrialization. Ambitious couples emulated upper-class behavioral patterns; the middle-class wife aspired to the role of gracious hostess and companion, producing a few high-quality children on the side. Since emulating upper-class life without an army of servants downstairs was quite labor intensive, a middle-class wife could be more than fully occupied meeting rapidly rising standards of child care, companionship, household sanitation, and gracious living.

³ While our study shows that the wife's work history positively affects thoughts of divorce, Cherlin (1979) shows that the husband's employment stability negatively affects divorce at low-income levels. Conceivably, the wife's work history would also positively affect divorce, and the husband's employment stability would negatively affect thought of divorce.

In turn, law and custom gave economically dependent wives little choice but to remain married, even if they heartily disliked their husbands. Very few wives receive alimony (U.S. Bureau of the Census 1979, p. 5). The wife's dependency also constrained the husband—especially if she put him through college or professional school—if he wanted to divorce her to marry a younger woman (see also Górecki 1966, p. 611). Thus, both husbands and wives in high-income families had reason to avoid divorce.

Today, women's economic roles are changing faster than men's. Women's massive entry into the paid labor force parallels men's during industrialization. High-income husbands are now much less likely than in the past to have nonemployed wives. In 1950 only 16% of wives in the top 5% of family income were employed; by 1970, 41% were, and the number continues to rise (U.S. Bureau of the Census 1976, p. 295). More wives can now weigh the costs and benefits of housework and market work. Shifting the balance of women's self-interest shifts, in turn, the balance for men. In the long run, as more wives enter the paid labor force, an important factor in the stability of the dual-earner family may be the extent to which dual earnings imply dual domestic labor.

A second finding suggesting change in factors that affect marriage is that the presence of children fails to deter mothers' thoughts of divorce and only the presence of young children deters fathers'. Should this finding occur in future studies of divorce, as we expect, it will make people nervous because they will think that parents care more about their own personal happiness than about their children's welfare. Changes in deeply embedded family norms arouse anxiety, even among social scientists. For example, the received wisdom typically describes the rise in the divorce rate as alarming. Perhaps we should turn the received wisdom on its head and be worried, instead, about the number of unhappy couples who, failing to attain divorce, continue to live together miserably. Similarly, if the presence of children fails to deter divorce, this may imply not that parents care less for their children but that they care more; parents can find alternatives to subjecting their children to life in an unhappy home.

One outcome seems probable, should the presence of children fail to affect a rising divorce rate: the norms that guide divorcing parents should experience vigorous growth. As all divorced and divorcing parents know, norms in this area are anemic guides to behavior. Enormous energy is needed to solve daily problems. Thirty years ago Kingsley Davis (1949) noted the lack of institutionalized patterns for dealing with children of divorce; the situation persists today (Cherlin 1978). We predict rapid development of norms concerning children of divorce in the near future.

Finally, our findings imply that an adequate sociology of marriage and family should begin with historical changes in how people make a living. The study of personality and sexual adjustment may provide useful infor-

mation for therapists, but it cannot explain long-term shifts in marriage patterns. In preindustrial societies marriage was situated in a web of economic reciprocities. Tearing that web decreased the durability of marriages, not because modern husbands and wives get along less well but because they have other options if they grow to dislike one another. The task for the sociology of marriage and family is to examine how these options emerged and to predict how women and men will react to them. Becker's focus on economic factors in marital instability paves the way for a sociological theory of marriage which, like the theory of the demographic transition, will explain how populations come to change their behavior when they confront a changed environment.

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Property Crime Rates in the United States: A Macrodynamic Analysis, 1947–1977; with Ex Ante Forecasts for the Mid-1980s¹

Lawrence E. Cohen
University of Texas at Austin

Marcus Felson
Carnegie-Mellon University

Kenneth C. Land²
University of Illinois at Urbana-Champaign

This paper presents several macrodynamic social indicator models of post–World War II trends in robbery, burglary, and automobile theft rates for the United States. A theory of the ways in which changes in criminal opportunity affect these Index Crime property crime rates is developed. Definitions and postulates are presented from which we derive a main theorem which states that, other things being equal, a decrease in the density of the population in physical locations that are normally sites of primary groups should lead to an increase in criminal opportunities and hence in property crime rates. Corollaries to the main theorem are presented and tested after operationalization of relevant independent and control variables such as the residential population density ratio, the unemployment rate, age structure, total consumer expenditures, and automobiles per capita. Stochastic difference equations, used to evaluate the theory, indicate that the models implied by the theory exhibit good statistical fit to the recorded property crime rates in question over the 26-year estimation period, 1947–72. In addition, these models provide reasonably accurate ex post forecasts of observed annual property crime rates over the five-year forecast period, 1973 through 1977. The paper concludes with a discussion of ex ante forecasted equilibrium levels of the three property crime rates for the mid-1980s implied by the estimated models. The forecasts indicate that the robbery and automobile theft rates should drop substantially in the 1980s from their recent levels, whereas the burglary rate may continue to grow or at least drop less.

Despite the fact that few criminologists anticipated the dramatic increases in reported crime that have occurred in the United States since 1960 (Wilson 1975, p. 59), surprisingly little research has been devoted to the

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² The authors' names are in alphabetical order.

problem of forecasting trends in official crime rates. Of the few existing studies, most use some form of demographic age-standardization technique to ascertain the extent to which changes in age structure affect levels of recorded crime rates (see, e.g., Christensen 1967; President's Commission on Law Enforcement and the Administration of Justice 1967). These few studies conclude consistently that changes in age structure *alone* can account for only a relatively modest portion of the increments in the general crime rates since 1960 in the United States (see also Sagi and Wellford 1968; Ferdinand 1970; Wellford 1973). The literature also contains a few multivariate analyses of trends in crime rates (see, e.g., Fox 1976; Land and Felson 1976). However, these studies tend to be based on relatively weak theoretical underpinnings and/or to be flawed on statistical grounds.³ Therefore, they may not provide a very reliable basis for the development of forecasts.

This paper presents several macrodynamic social indicator models⁴ of post-World War II trends in three property crime rates⁵ for the United States: robbery,⁶ burglary, and automobile theft. The models, to be described below, rest on a simple *opportunity theory* according to which decreases in the density of population in physical locations customarily occupied by persons having primary-group relationships (e.g., residences) produce increases in the risk of property crimes, other things being equal. Although this theory is stated here for the context of property crimes, it is generally applicable to the risk of violent crimes as well (for some insights on

³ E.g., Fox (1976) takes annual changes in both the aggregate Index Crime property crime rate and the aggregate index violent crime rate in the United States since 1950 as functions of two exogenous forces: the proportion of young black males in the population and the consumer price index. This model specification, however, ignores the obvious fact that the proportion of young black males in the population cannot account for the apparently dramatic rise in white and female crime rates during this period. The use of an inflation index as the only other variable which drives crime rates up also seems to be questionable on theoretical grounds, particularly with respect to the violent crime rate. Although Fox (1976) achieves high coefficients of determination (as often occurs in time-series analysis), the diagnostic statistic of his estimated equations indicate that the null hypothesis of zero autocorrelation among the disturbances cannot be accepted. This also suggests model misspecification. While the models of Land and Felson (1976) do not suffer similar statistical deficiencies, they are specified on the basis of existing empirical generalizations as opposed to a consistent theoretical statement.

⁴ Social indicators can be defined broadly as measures of social conditions, while social indicator models are formal systems designed to explain changes in social indicators (see Land and Spilerman 1975). Macrodynamic social indicator models consider how one social change affects another; they employ macro-level data and apply macro social theory in modeling changes in the indicators (see Land and Felson 1976; Land 1979).

⁵ We do not analyze one additional Index Crime that is traditionally considered a property crime—larceny of \$50 and over—because the Federal Bureau of Investigation ceased making annual estimates of its national rate after 1972. The development of forecasts for this crime would be a somewhat moot exercise inasmuch as there is no continuing baseline of observations with which the forecasts can be compared.

⁶ Although robbery is usually classified as a violent crime, it will be treated here as a property crime because it involves the transfer of property.

the latter type of application, see Cohen and Felson [1979]). Moreover, while this theory is probably not the only possible explanation of trends in crime rates that is consistent with existing data, it does possess three desirable attributes. First, it is parsimonious and does not require any highly implausible or counterfactual assumptions. Second, the models to which it leads exhibit a good statistical fit to recorded property crime rates over the 26-year estimation period, 1947 through 1972. Third, these models also provide reasonably accurate ex post forecasts of observed annual property crime rates over the five-year forecast period, 1973 through 1977.

The length of this forecast period, together with the fact that observed values of the property crime rates do not exhibit "simple linear trend" behavior over these years, provides substantial evidence that the macro causal mechanisms identified by our theory are relatively stable characteristics of U.S. society and not appropriate for the forecast period alone. Therefore, we argue that our theory, and the models to which it leads, may provide a basis for more accurate forecasts of official crime rates. In fact, we conclude with a discussion of the ex ante forecast equilibrium levels of property crime rates for the mid-1980s implied by our estimated models. These are of value because, whatever their shortcomings as measures of the true rates of criminal events in the society, official crime statistics are significant social indicators of the volume of cases brought to the attention of the criminal justice system and of the reaction of the system thereto (Seidman 1978).⁷ Therefore the effectiveness of planning for the development of the criminal justice system depends, in part, on the accuracy of forecast crime rates. Before describing our approach to such forecasts, we first review the post-World War II record of annual reported property crime rates for the United States and discuss related issues of quality and composition of the data on which this record is based.

THE RECORD OF ANNUAL CHANGES IN U.S. PROPERTY CRIME RATES, 1947-77

Two published sources of time series on official crime rates in the United States are *Social Indicators: 1973* (OMB 1974) and the *Uniform Crime Report* (FBI 1947-77). The latter source, which we refer to as the *UCR*, presents fairly consistent time-series estimates of six of the seven Index Crime rates (nonnegligible homicide, forcible rape, aggravated assault, robbery, automobile theft, burglary) for the years 1960-77. The former, which we call *SI-73*, publishes FBI estimates of time series for these six rates plus a consistent series of estimates of larcenies of \$50 and over for the years 1947-72. Each of these sources presents estimated crime rates

⁷ Official crime rates also may affect public opinion (see, e.g., Hindelang et al. 1976, tables 2.1-2.24).

derived from weighted averages of state-city-size-specific rates. At various times, the FBI recalculates these estimates using new and presumably better information.⁸ Unfortunately, the *SI-73* time series have not yet been updated and are somewhat inconsistent with the *UCR* estimates. However, for the robbery, burglary, and automobile theft rates, the inconsistencies between the *SI-73* and *UCR* estimates are minor. For instance, the correlations between time series from the two sources for the years 1960-72 are .9999 for each of these three rates. Nonetheless, in order to extend the *SI-73* series beyond 1972 in such a way as to be consistent with the years that the two sources have in common, some adjustment is called for. Our procedure consisted of regressing the *SI-73* rates on the corresponding *UCR* rates over the 1960-72 period and then using the reported *UCR* rates for 1973-77 to calculate estimated extensions of the *SI-73* time series through 1977.

The results of this "splicing" and updating of the *SI-73* series are presented in figures 1-3. For comparison, these figures also exhibit the annual *UCR* estimates over the last five years. As can be seen from the figures, our

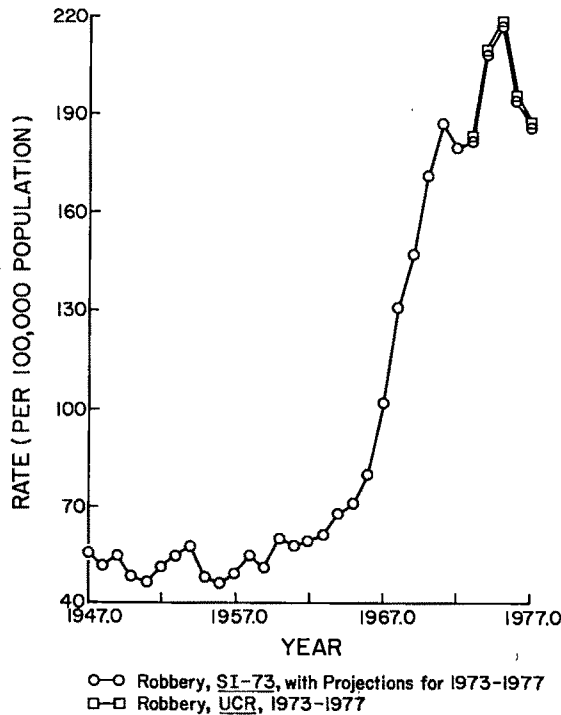


FIG. 1.—Robbery rate, United States, 1947-77

⁸ This practice of periodically revising estimates of past values of time-series data on the basis of more recent information is not unique to official crime statistics; for instance, it is a standard practice for the gross national product time series and its components.

updated *SI-73* estimates are very close to the *UCR* estimates for 1973-77 and display similar patterns of year-to-year behavior.

Before taking the series of estimated rates in figures 1-3 as indicative of trends in the frequencies of corresponding criminal events in the society, two alternative interpretations must be considered. The first is that trends in these data reflect largely the organization of the police agencies which supply the FBI voluntarily with data on known Index Crime violations. For example, there appears to be little question that particular cities and states vary in quality of reporting at any given time and that localities sometimes change quite suddenly the quality of the data they supply the FBI (see Seidman and Couzens 1974, pp. 459-66). Of far greater concern to most recent criminological inquiries is the possibility that trends in official crime statistics are due primarily to trends in the rates at which offenses are reported to the police. If either of these hypotheses can account for most of the variance in the trends reported in figures 1-3, any attempt to identify other social forces that affect crime rates is a futile exercise.

While administrative phenomena need to be considered in any cross-sectional analysis of variations in crime rates among particular localities, they are less likely to be a major factor in analyses of trends over time in

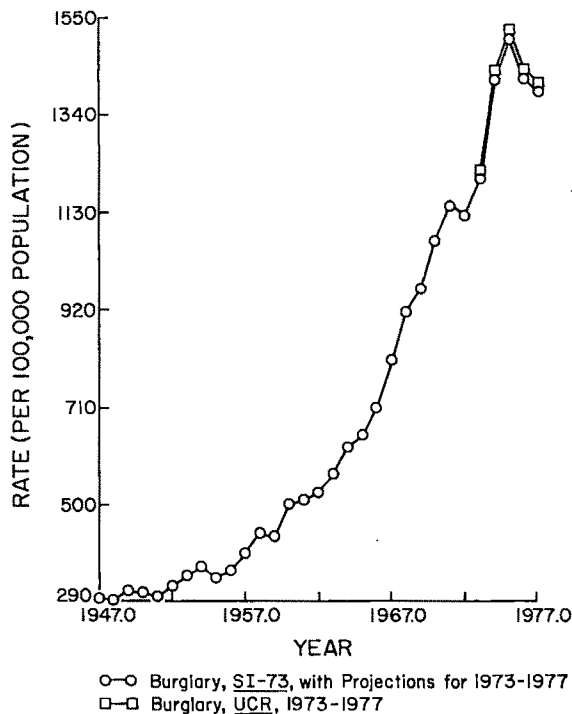


FIG. 2.—Burglary rate, United States, 1947-77

aggregate national crime rates for two reasons. First, the FBI, in a conscious effort to estimate national crime rates which are minimally affected by organizational idiosyncrasies, routinely excludes from its estimates localities which are notorious for questionable reporting systems.⁹ Second, many of the remaining inconsistencies average out in the aggregate national estimated crime rates since different police agencies reorganize their reporting systems in different years, thus affecting official crime statistics in different directions. To be sure, during some years police reorganization occurs in several localities; this was particularly true of the early 1960s. However, examination of the time-series data on Index Crime rates indicates that the FBI's estimated crime rates continued to rise several years after the latter period. Even cities which had respected crime-reporting systems as early as the 1950s (e.g., Los Angeles) have experienced rather dramatic increases

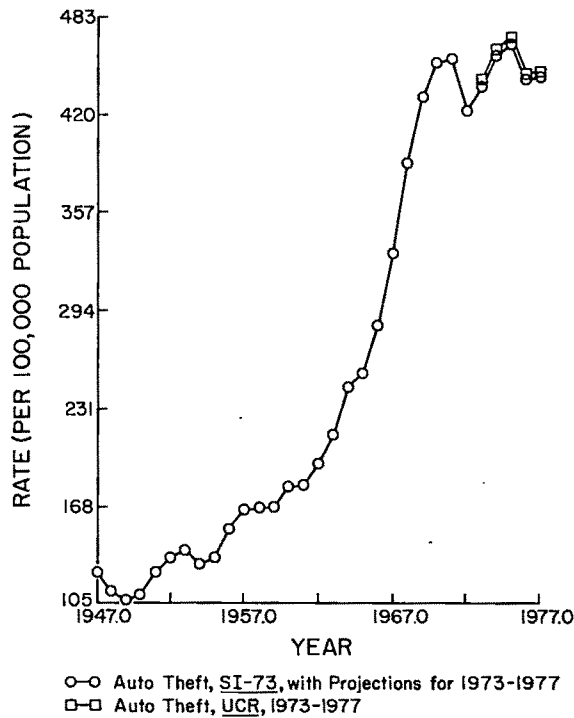


FIG. 3.—Automobile theft rate, United States, 1947-77

⁹ Political interference with official crime statistics may sometimes occur, particularly within specific localities. Seidman and Couzens (1974) note that "downgrading" of offenses into non-Index Crimes is the most common method for police to give the appearance of declining crime rates. This is easiest to carry out with such crimes as larceny: valuation of a theft as \$49 rather than \$51 keeps it out of the crime index. However, downgrading is more difficult to carry out with respect to robbery, burglary, or automobile theft.

in crime rates since that time. In general, then, it is doubtful that *national* trends in Index Crime rates are primarily administrative in origin, even though certain local trends may be largely so. In any event, such risks are probably less serious in aggregate time-series analyses such as those presented in this paper.

The possibility that trends in official crime statistics can be accounted for primarily by trends in the rates at which offenses are reported to the police has motivated extensive victimology research designed, in part, to determine the extent of underreporting in the property and violent crime data compiled by the FBI (see Hindelang [1976] and Nettler [1978] for reviews of the victimization literature). One consistent finding in victimization studies is that automobile theft is the most completely reported property crime (Skogan 1976). Given that automobiles are expensive pieces of property and that their theft must be reported for victims to collect insurance settlements, this finding is intuitively meaningful. More generally, victimization studies show consistently that the three property crimes studied in this paper are ordered in terms of completeness of reporting from automobile theft (about 69% of offenses reported) to robbery (about 53% of offenses reported) to household burglary (about 48% of offenses reported). These are conservative estimates based on a four-year average (1973-76) of national victimization surveys (USDJ 1978).

Could the reported index property crime trends shown in figures 1-3 be accounted for by increases in report rates with *no* increases in the numbers of offenses committed? The figures show that, for this to be so, the report rates for robbery, burglary, and automobile theft would have had approximately to triple since about 1960. Increases in report rates of this order of magnitude, while arithmetically feasible, imply implausibly low report rates in the period 1947-60 (on the order of 17% for robbery, less than 16% for burglary, and about 23% for automobile theft). Therefore, the consensus among criminologists who have studied this problem is that increases in report rates, taken by themselves, have not been sufficiently large to account for all, or even most, of the variance in reported index property crime rates since 1960 (for a careful survey of data and arguments related to this issue, see Nettler [1978, chap. 2]).

In brief, we conclude that administrative reorganization and increases in report rates can account for only a small fraction of the variation in the trends of the estimated robbery, burglary, and automobile theft rates reported in figures 1-3. Therefore, in subsequent sections we state hypotheses and specify models that are consistent with the fluctuations of each of the series in these figures around relatively stable equilibria in the late forties and early fifties, the subsequent monotonic increases beginning in the mid-fifties to early sixties, and the roughly stable equilibria with fluctuations beginning after 1972.

A THEORY OF CHANGES IN CRIMINAL OPPORTUNITY AND PROPERTY CRIME RATES

A substantial body of descriptive and analytic evidence supports the general proposition that a large proportion of property crimes is precipitated by situational opportunities encountered by potential offenders (see, e.g., Gould 1969; Jackson 1969; Gibbons 1971; Feeney and Weir 1973; Mansfield, Gould, and Namenwirth 1974; Reppetto 1974; Simon 1975; Hindelang, Gottfredson, and Garofalo 1978). Even when offenders deliberately seek out property targets an increased number of situational opportunities will provide them with circumstances favorable to criminal activity. Consequently, it appears reasonable to develop an explanation for changes in post-World War II U.S. property crime rates by concentrating on how the circumstances favorable to property crime have changed. There are many ways in which a theory of opportunity determinants of property crime rates could be constructed. Obviously, some choices must be made. The approach to be pursued here is embodied in the following definitions and behavioral assumptions.¹⁰

The first definition makes use of Glaser's (1971, p. 4) notion of "predatory violations" as being those illegal acts in which "... someone definitely and intentionally takes or damages the person or property of another." We follow Cohen and Felson (1979) in adding to this definition the requirement of direct physical contact. This yields a concept of *direct-contact predatory property crimes* as predatory violations involving direct physical contact between at least one offender and at least one property object. From this definition, it is clear that the three index property crime rates of interest in this paper—robbery, burglary, and automobile theft—involve direct-contact predatory property crimes.

A second basic definition pertains to the *minimal components* of this class of crimes. That is, by definition, direct-contact predatory property crimes require the simultaneous interaction of three elements: (1) one or more *offenders*, (2) one or more suitable *property targets*, and (3) the *absence of effective guardians*. In this definition, "suitable property targets" are property objects accessible to offenders and physically suitable for removal, and "effective guardians" are persons able to prevent violations from occurring either by their physical presence alone or by some form of direct action (for a more lengthy discussion of these minimal elements, see Cohen and Felson

¹⁰ The first two definitions in this section constitute direct applications to property crimes of definitions introduced in Cohen and Felson's (1979) "routine activity" approach to the analysis of crime trends. Although that paper contains some of the basic definitions utilized here, together with many relevant empirical data and a related explanation of crime trends in contemporary American society, it does not attempt to present a formal theoretical statement. In contrast, the present paper moves in the direction of a formal conceptualization of the effect of social changes on one component of direct-contact predatory crimes identified by Cohen and Felson (1979)—the absence of guardians.

[1979]). Examples of the former are money, jewelry, electronic equipment, and automobiles, whereas examples of the latter are housewives, pedestrians, private security guards, and law-enforcement officers.

The third definition concerns the types of physical locations in which property targets are found in contemporary American society. While an enumeration could be very long and detailed, for present purposes the following simple classification is sufficient. Property targets are *physically located* (1) in residential locations, (2) in nonresidential locations, or (3) on the person of individuals located either in residential or nonresidential locations or in transit between them. In this definition, the term "residential locations" refers to domiciles, while the term "nonresidential locations" means all nondomestic places of work or leisure, such as commercial establishments, schools, churches, and parks.

In order to use these definitions to explain how changes in criminal opportunity affect index property crime rates, we need to combine them with one or more behavioral assumptions. Although a variety of assumptions can be so utilized, we choose, in the tradition of the studies cited in the first paragraph of this section, to state propositions relating to the ways in which structural transformations facilitate or impede opportunities to engage in illegal activities. In other words, we focus on how social changes affect the rate of simultaneous convergence of the minimal components of direct-contact predatory property crimes in physical locations containing property targets.

Assumption 1

Other things being equal, direct-contact predatory property crime offenders prefer property targets located in sites with fewer, rather than more, potential guardians.

Not only is this assumption deducible from a conception of property crime offenders as engaging in rational choice behavior (see, e.g., Nettler 1978, chaps. 9 and 10), but also it is consistent with available criminological evidence (see, e.g., Jackson 1969; Scarr 1972; Letkemann 1973; Reppetto 1974; Pope 1977*a*, 1977*b*; Hindelang et al. 1978; Cohen and Felson 1979).¹¹

Assumption 2

Other things being equal, persons who are related to an individual by secondary-group ties or by no stable social relationships at all and who do not themselves have norm-enforcing role obligations are less likely than

¹¹ Of course, the complexity of criminal motivation must be acknowledged as well as the fact that some offenses are planned well beforehand while others may be a spontaneous response to unsought opportunity.

persons related to the individual by primary-group ties to act as guardians for the individual's property.

The concepts utilized in this assumption relate to traditional sociological notions of primary and secondary groups (Cooley 1909). Furthermore, substantial evidence in support of the behavioral assertions in this assumption has accumulated in victimization surveys (for reviews of this literature, see Hindelang et al. [1978]; Cohen and Felson [1979]).

The foregoing definitions and behavioral assumptions can be combined to derive a number of propositions about how social changes affect property crime rates. In this paper, however, we restrict our attention to the following main proposition and two of its corollaries.

Main theorem.—Other things being equal, a decrease in the density of a population in physical locations that are sites of primary-group routine activities (i.e., role-directed behaviors) produces an increase in criminal opportunity and hence an increase in the rate of occurrence of property crime violations.¹²

Corollary 1.—Other things being equal, a decrease in the density of a population in residential locations produces an increase in criminal opportunity and hence an increase in the rate of occurrence of property crime violations.

Corollary 2.—Other things being equal, an increase in the number of persons in transit locations produces an increase in criminal opportunity and hence an increase in the rate of occurrence of property crime violations.

The arguments necessary to establish these propositions are straightforward. For the main theorem, note that a deconcentration of a population in physical locations that are sites of primary-group routine activities decreases the size of the population of potential primary-group guardians which, in turn, raises the attractiveness of these locations to property crime offenders according to our first behavioral assumption. Thus, other things being equal, we expect such a social change to produce an increase in property crime rates. Of course, this implies a corresponding increase in the concentration of the population in physical locations that are sites of activities among unrelated individuals or individuals related by secondary-group ties which should, on the basis of the same behavioral assumption, lower the attractiveness of these sites to property crime offenders. However, this effect is offset by the lower rate of guardianship in such locations asserted by our second behavioral assumption. Thus, in the absence of specific quantitative information about the relative sizes of these various effects, we assume that the latter effect is essentially neutral and that, when

¹² This proposition is not intended to imply any psychological relationship between population density and criminal motivation. As noted in the argument for corollary 1, we seek only to link guardianship against crime to a specific type of density, namely, the concentration of people within residential as opposed to nonresidential locations during specific time intervals.

it is combined with the former effect, the result is a net increase in index property crime rates.

Consider next the derivation of corollaries 1 and 2. Corollary 1 requires only the recognition that residential locations are almost exclusively locations of primary-group activities; the statement then follows by direct application of the main theorem. Note that no similar statement can be made regarding the effect of deconcentration of a population in nonresidential locations on property crime rates, because nonresidential locations are heterogeneous with respect to the prevalence of activities among unrelated individuals and primary and secondary groups. Thus, whereas nonresidential locations contain work groups which may be related by primary-group ties, they also are sites of activities among unrelated individuals such as those engendered by business transactions among individuals not otherwise socially related to each other. In contrast, our third category of physical locations—transit locations—includes mainly locations in which the individuals present are unrelated to each other. Exceptions may exist in small towns or in local neighborhoods within larger cities, but these contain only a small fraction of the total number of transit locations. Therefore, by applying the second of our behavioral assumptions together with the main proposition, we obtain corollary 2. In the next section, we identify two aggregate indexes by which we operationalize these corollaries. We also specify some indexes of conditions that should be controlled under their "other things being equal" clauses.

RESULTS OF EMPIRICAL ESTIMATION, 1947-72

The Functional Form to Be Estimated

In this section, we report empirical results obtained by fitting single-equation macrodynamic structural models to the three index property crime series exhibited in figures 1-3 over the 26-year period, 1947-72. The models to be fitted have the general form

$$R_t/R_{t-1} = e^{\alpha} \cdot R_{t-1}^{\beta-1} \cdot X_{1t}^{\gamma_1} \cdot X_{2t}^{\gamma_2} \cdot \dots \cdot X_{Kt}^{\gamma_K} \cdot \epsilon_t, \quad (1)$$

where R_t and R_{t-1} denote estimated levels of reported index property crime rates for years t and $t - 1$, respectively; e is the base of the natural logarithms; X_{1t} , X_{2t} , \dots , X_{Kt} denote the values of K exogenous variables in year t ; ϵ_t is the value of a stochastic disturbance in year t ; and α , β , γ_1 , γ_2 , \dots , γ_K are coefficients to be estimated. Equation (1) is written in multiplicative form because our definition of the minimal components of direct-contact predatory property crimes asserts that these components interact simultaneously. In other words, each of the minimal components is a necessary, but not sufficient, condition for the occurrence of a property crime; therefore, a multiplicative, rather than linear-additive, functional

form is implied (see, e.g., Land 1970, p. 264). This assertion regarding functional form is, of course, testable, and, in the text below, we report some results on comparative goodness of fit between linear-additive and multiplicative forms.

Note that, by taking the natural logarithms (\ln) of both sides of equation (1), we obtain the log-linear functional form

$$\ln R_t = \alpha + \beta \ln R_{t-1} + \gamma_1 \ln X_{1t} + \gamma_2 \ln X_{2t} + \dots + \gamma_K \ln X_{Kt} + \ln \epsilon_t. \quad (2)$$

In this form, equation (2) is recognizable as a linear (in the logarithms) first-order stochastic difference equation; alternatively, it can be described as a first-order autoregressive regression equation. Land (1979) has identified this as an appropriate class of single-equation models for time series of occurrence/exposure rates (such as index property crime rates) that change from period to period according to a period-inhomogeneous Markov process. For details of the arguments leading to this lag structure, the reader is referred to Land (1979). Substantively, in the present context, the lag-term coefficient β will tend to zero if changes in the exogenous variables of equation (2) have a fairly rapid impact on the index property crime rates. However, if the index property crime rates studied here respond more slowly to changes in the exogenous variables than during the base period of the model (single years in the present paper), $|\beta|$ will be greater than zero; in other words, in this case the effects of changes in period t in the values of the exogenous variables on an index property crime rate are distributed over several subsequent periods as opposed to being fully realized in period t . This suggests the existence of complex social learning and diffusion processes that affect the transmission and persistence of these criminal behaviors.

Definitions of the Exogenous Variables

Before we can estimate models of the form of equation (2) for each of the three property-crime-rates series exhibited in figures 1–3, we must use our theoretical statement to specify the substantive contents of the exogenous variables in the equation.

Consider first the operationalization of the density concept used in corollary 1. It is clear that for this purpose a “residential population density index constructed in the traditional human ecology or geographical sense (Thomlinson 1965, pp. 271–72) of persons per unit of physical area (acre, square mile) of residential locations in the population under study would be most desirable. It is clear also that the data necessary to construct such an index in annual time-series form for the post-World War II U.S. population do not exist. Therefore, we use an alternative population-based density

index which adds to the number of female labor force participants with husband present the total number of households which are non-husband-wife households; this sum is then divided by the total number of households to provide an index of household exposure to risk of residential property crime incurred either because people live alone or because labor force participation removes the wife from the household during her working hours. Cohen and Felson (1979) label this index an "activity ratio." Actually, since an increase in the numerator measures a decrease in the number of persons likely to be physically present in household domiciles *on the average* (over a given time period such as a year), and the denominator is the total number of households, the index can be construed as a household population density ratio. Therefore, since in this paper households are tied to residential locations we call this index a "residential population density ratio."¹³ Note that sufficient data exist from census sample surveys to compute annual values of this ratio for the post-World War II U.S. population. Note also that, because of the way the index is constructed, the inverse relationship stated in corollary 1 will be corroborated by a *positive* estimated coefficient.

Second, we use the unemployment rate as an index which is applicable to both corollaries. It can be argued that periods of high unemployment tend to remove some persons from work activity and therefore to reduce their exposure to property crime victimization in transit locations outside their residential neighborhoods (corollary 2). It can be argued also that an economic downturn, by reducing overtime work and the money available for leisure, contributes to the concentration of activities within or proximate to households (corollary 1). In either case, we are led to expect an inverse relationship between the unemployment and Index Crime property crime rates.¹⁴

With respect to the "other things being equal" qualifiers of our theoretical propositions, three additional variables are controlled in our empirical analyses.

¹³ Since the "female labor force participants with husband present" part of the numerator of this density index is not definitionally a subset of the denominator (more than one such husband-wife pair could belong to the same household), we use the term "ratio" rather than "rate" to refer to this index (for a systematic discussion of the difference between demographic rates and ratios, see, e.g., Land [1979]).

¹⁴ On first glance, it might appear that this derivation contradicts the well-known positive association of economic deprivation and crime rates found consistently in cross-sectional studies (for an extensive review of studies pertaining to this finding, see Reiss [1976]). However, it is possible for the result derived here to hold: an increase in the unemployment rate results in a *net* decrease in property crime rates *on the average for the society as a whole*, while, at the same time, a positive association is found *cross-sectionally* both before and after the increase in unemployment between the static distributions of the population by economic deprivation and property crime victimization. This raises the interesting research question of whether the latter association increases or decreases with increases in the unemployment rate for the society as a whole; we are not aware of any direct empirical evidence concerning this question.

To begin with, we control one offender-related variable, namely, the proportion of the U.S. population aged 15–24 in each year of our estimation period. Although some other age categorizations might work almost as well, this age group has been found consistently to be among the most active with respect to the production of criminal offenders (Gibbons 1977, p. 111). To some extent, this index is also victim related, in the sense that teenagers and young adults are at greater risk of property crime victimization than are older adults. However, evidence from victimization surveys shows that the age gradient for property crime victimization is much flatter than that for property crime offenders and does not decline very steeply until after age 50 (Hindelang 1976, p. 278). Therefore, the 15–24 age range used here is more offender related than victim related.

To measure trends in the availability of suitable property targets, we utilize two indexes. First, to index the volume of valuable property targets available for burglaries, we use the total consumer expenditures in year $t - 2$ for durable goods other than automobiles in constant (1967 = 100) dollars. The two-year lag is based on empirical evidence about how consumers maintain and update their current stock of consumer durables (Juster 1961). Second, we control the number of automobiles per capita, which may affect auto theft rates.

Sources for the time-series data on these variables, as used in the present analysis, are reported in the Appendix.

Statistical Methods of Estimation and Hypothesis Testing

Tables 1–3 report the statistical results for fitting functions of the form of equation (2) to the robbery, burglary, and automobile theft series, respectively, over the 26-year period from 1947 to 1972 with exogenous variables as defined in the preceding subsection.

Some of the equations in tables 1–3 contain autoregressive terms. For such equations, it is well known that the OLS estimates are biased in small samples (as is true of all other known estimators); also, they are consistent in large samples only if the disturbances are not autocorrelated (see, e.g., Johnston [1972, pp. 303–7] for proofs of these properties). Therefore, our estimation strategy is (1) to use OLS estimators to obtain initial estimates of the equations, (2) to subject the computed OLS residuals to tests for autocorrelation, and, if the latter indicates the presence of autocorrelated disturbances, (3) to reestimate any of the equations that exhibit autocorrelated disturbances by a combined instrumental-variables-generalized-least-squares method that is consistent in the presence of autocorrelated disturbances (Hibbs 1974).

It is clear that tests for autocorrelated disturbances play a crucial role in this estimation strategy. For this reason, we report both the well-known

Durbin-Watson (1950, 1951) statistic (D-W) and the more recently formulated Durbin (1970) *h*-statistic in tables 1-3. The D-W was originally formulated to test for the presence of autocorrelation in regression equations that do not have lagged dependent variables, and it is well known that the statistic is biased toward acceptance of the null hypothesis of zero autocorrelation of the disturbances when a lagged dependent variable is present (see, e.g., Johnston [1972, p. 310] for a demonstration). In contrast, Durbin's *h*-statistic was constructed explicitly to test for autocorrelated disturbances in equations that contain lagged dependent variables. Nevertheless, simulation studies have shown that *h* does not perform very well in small samples and that the D-W is as good as, or better than, *h* in small samples when its "upper bound" is used to define the critical region of the test (Taylor and Wilson 1964; Kenkel 1974). Therefore, because neither statistic is clearly superior to the other on both theoretical and empirical grounds, we report both (when appropriate) in tables 1-3. Our criterion for failing to reject the null hypothesis of zero autocorrelation of the disturbances is that neither of these statistics should be statistically significant at the .01 level.¹⁵

Description of the Estimated Equations

Table 1 presents our estimated equation for the robbery rate. Note that the residential population density ratio has the largest *t*-ratio among the exogenous variables.¹⁶ The coefficients of the unemployment and age-structure indexes also are statistically significant. Similarly, the constant of the equation is significantly positive, indicating that some number of robbery events should persist even if the other variables went to zero. The equation explains over 96% of the year-to-year variance in robbery rates and has a standard error of estimate of about 0.08 natural log units, which indicates approximately an 8% error expected for any given robbery rate estimate. The null hypothesis of zero autocorrelation among the disturbances is easily accepted.

¹⁵ As a guideline for interpreting the 0.01 values of the D-W reported in tables 1-3, its upper bounds with three, four, and five independent variables (including the constant) are as follows: with 20 df, 1.27, 1.41, and 1.57, respectively; with 21 df, 1.27, 1.41, and 1.55; and with 22 df, 1.28, 1.40, and 1.54. Note that these values define one-tailed critical regions below which an observed value of the D-W indicates positive autocorrelation of the disturbances. To test for negative autocorrelation, the corresponding critical values are defined by subtracting these values from 4.0, and observed values of the D-W above the resulting critical values are taken to indicate negative autocorrelation of the disturbances. Since the *h*-statistic is asymptotically distributed as a normal deviate, its corresponding one-tailed critical regions are defined by ± 2.326 .

¹⁶ Because of the relatively modest degrees of freedom involved in the estimation of the equations reported in this paper, we use $\alpha = .10$ as a criterion for statistical significance for the regression coefficients. For df = 20, 21, and 22, this requires *t*-values greater than 1.325, 1.323, and 1.321, respectively, for a one-tailed test.

Substantively, equation (3) is quite interesting, for it indicates that the robbery rate increases with greater proportions of adolescents and lower residential population density ratios, yet is fairly slow to respond to changes in these variables owing to the strong lagged term.¹⁷ The estimated coefficient for the latter indicates an adjustment of the robbery rate of about 0.22 natural log units per year toward changes in its equilibrium value due to changes in the exogenous variables. Equation (3) also suggests an impact of unemployment on robbery of modest statistical significance but in the direction predicted by our theory. The main contribution of the unemploy-

TABLE 1
ESTIMATED ROBBERY RATE EQUATION,
UNITED STATES, 1947-72

Predetermined Variables	Estimated Equation (3)
Constant.....	2.5325 (2.5532)*
In robbery rate lagged ($t - 1$).....	.7847 (7.4996)*
In residential population density ratio (t).....	.6886 (3.2067)*
In unemployment rate (t).....	-.0985 (1.3620)*
In proportion aged 15-24 (t).....	.4915 (1.7273)*
Summary Statistics	
\bar{R}^29652
SEE.....	.0846
D-W.....	2.2183*
h	-.6579*
df.....	21

NOTE.—Estimates are metric regression coefficients estimated by OLS; figures in parentheses are t -ratios calculated from the coefficients divided by their standard errors; \bar{R}^2 = squared multiple correlation coefficient adjusted for degrees of freedom; SEE = standard error of estimate; D-W = Durbin-Watson autocorrelation statistic; h = Durbin's autocorrelation statistic for equations with lagged endogenous variables; df = degrees of freedom.

* Fail to reject the null hypothesis of zero autocorrelation of disturbances at $\alpha = .01$.

* Significantly nonzero at $\alpha = .10$, one-tailed test.

¹⁷ Unlike the burglary and automobile theft equations described in the text below, this equation does not contain an index of property target availability that is distinct from the density and unemployment indexes. This is due to the fact that the probability of victim confrontation (or confrontation of an employee of the victim, as in commercial robbery) is, by definition, 1.0 for robbers, but considerably less than 1.0 for burglars and automobile thieves. Consequently, insofar as our density and unemployment indexes control for major temporal variations in exposure to risk of robbery, they also control for variations in target availability. For additional discussion of property targets and robbery, see Conklin (1972, pp. 80-84).

ment rate is to take into account the effect of the business cycle on levels of exposure to the risk of robbery.

Table 2 exhibits two estimated equations for the burglary rate. Note first that the estimated coefficient of the lagged burglary rate is statistically insignificant in equation (4). Hence, we infer that the burglary rate adjusts sufficiently quickly to changes in the levels of the exogenous variables in equation (4) that a dynamic specification is unnecessary. Therefore, after the autoregressive term is deleted, reestimation results in equation (5). This equation explains 99% of the year-to-year variance in the burglary rate with strongly significant positive effects of the residential population density ratio and the proportion of the population aged 15-24. Although the property targets' availability index has a somewhat smaller *t*-ratio, its algebraic sign is consistent with theoretical expectations. That is, we infer that increases in the consumption of nonautomobile durable goods feeds the burglary rate. Note, however, that the estimated coefficient of the unemployment rate in equation (5) is significantly positive (by a two-tailed test because of the reversal of its algebraic sign), in contradiction to the negative sign expected on the basis of our theoretical arguments. Apparently the supply

TABLE 2
ESTIMATED BURGLARY RATE EQUATIONS, UNITED STATES, 1947-72

PREDETERMINED VARIABLES	ESTIMATED EQUATIONS	
	(4)	(5)
Constant.....	7.2120 (3.0095)*	7.9655 (9.6868)*
In burglary rate lagged ($t - 1$).....	.0887 (.3358)	...
In residential population density ratio (t)....	2.2062 (3.4934)*	2.4118 (15.8703)*
In unemployment rate (t).....	.0633 (1.4069)	.0701 (1.7928)**
In proportion aged 15-24 (t).....	.7627 (2.5552)*	.8494 (5.8031)*
In consumer durable expenditures (nonauto- mobile), constant 1967 dollars, ($t - 2$)....	.4029 (2.3415)*	.4306 (2.9190)*
Summary Statistics		
\bar{R}^29900	.9904
SEE.....	.0453	.0443
D-W.....	2.1072 ^a	2.0357 ^a
h	^b	...
df.....	19	20

NOTE.—See table 1.

^a Fail to reject the null hypothesis of zero autocorrelation of disturbances at $\alpha = .01$.

^b Durbin's *h*-statistic is not defined for this equation because the product of the variance of the autoregressive coefficient and the sample size is greater than 1.0.

* Significantly nonzero at $\alpha = .10$, one-tailed test.

** Significantly nonzero at $\alpha = .10$, two-tailed test.

of physical locations with relatively few effective guardians is sufficiently large that increases in the unemployment rate of the order of magnitude observed in our post-World War II sample period, even while resulting in marginal increases in the number of potential guardians, do not lead to the decreases in the burglary rate predicted by our theory. Indeed, for burglary, the unemployment rate seems to behave more as an index of the presence of offenders than as an index of variations in criminal opportunity due to the presence or absence of guardians.

The summary statistics for equation (5) show that the null hypothesis of zero autocorrelation of disturbances need not be rejected and that a 4%-5% error in burglary rate forecasts can be expected from the equation. The constant term is positive and highly significant, indicating that a nonzero level of burglary would be expected even if the exogenous variables assumed zero values. Finally, it should be emphasized that equation (5) is an algebraic rather than a difference equation. As such, it demonstrates that the high \bar{R}^2 values obtained by macrodynamic models are not necessarily the spurious result of the inclusion of lagged dependent variables.

Consider next the automobile theft rate, for which table 3 presents two equations. Equation (6) indicates that the rate of automobile registrations

TABLE 3
ESTIMATED AUTOMOBILE THEFT RATE EQUATIONS,
UNITED STATES, 1947-72

PREDETERMINED VARIABLES	ESTIMATED EQUATIONS	
	(6)	(7)
Constant.....	.3379 (.0692)	4.9878 (5.9969)*
ln auto theft rate lagged ($t-1$).....	.5282 (5.6019)*	.4890 (5.9256)*
ln residential population density ratio (t)....	1.0568 (1.2104)	1.7998 (8.6798)*
ln unemployment rate (t).....	-.1660 (4.6222)*	-.1719 (4.8976)*
ln proportion aged 15-24 (t).....	.3823 (2.4397)*	.3714 (2.3906)*
ln autos per capita (t).....	.3766 (.8765)	...
Summary Statistics		
\bar{R}^29925	.9926
SEE.....	.0413	.0411
D-W.....	1.6166 ^a	1.7694 ^a
h	1.1150 ^a	.6480 ^a
df.....	20	21

NOTE.—See table 1.

^a Fail to reject the null hypothesis of zero autocorrelation of disturbances at $\alpha = .01$.

* Significantly nonzero at $\alpha = .10$, one-tailed test.

per capita does not have a statistically significant net effect upon the auto theft rate. This finding differs from that of Mansfield et al. (1974); in part, it is due to the collinearity of the automobile registration rate and the residential population density ratio over the estimation sample period. Equation (7) trims away the automobile registration variable and indicates that approximately half of the effect of the independent variables upon the automobile theft rate is realized within year t , while the other half is distributed over a longer period of time. The significantly positive constant coefficient in equation (7) implies that, at zero levels of the exogenous variables, there still would be a nonzero level of automobile theft, as was true for the robbery and burglary equations. The age-structure effect in equation (7) is statistically significant; yet its t -ratio is much lower than those of the density and unemployment variables. The highest t -ratio in this equation is for the effect of the residential population density ratio. Similarly, the coefficient for unemployment has a large t -ratio and a negative sign, indicating (in support of corollary 2 of our theory) that persons not working (or working less during a business slump) may be less likely to expose their automobiles to illegal removal. Equation (7) explains 99.3% of the variance in auto theft rates, its standard error of estimate implying approximately 4.1% expected error in auto theft rate predictions (i.e., 0.041 natural log units). The D-W and h -statistics indicate that we easily can accept the null hypothesis of zero autocorrelation of disturbances in this equation.

In general, equations (3)–(7) in tables 1–3 appear to perform rather well on both substantive and statistical grounds over the estimation period. Moreover, while space is not available to give the numerical estimates, the log-linear functional form of these equations is superior to the linear-additive form for the same equations; the latter form exhibits autocorrelated disturbances, suggesting misspecification. Thus, the multiplicative structure specified in our theory also is supported. Yet another major test of the equations pertains to how well they perform when forecasting reported property crime rates in a postestimation sample period. This question is taken up in the next section.

EX POST FORECASTING ANALYSES, 1973–77

Recall that we did not use all of the available sample time points of the property crime rates charted in figures 1–3 in estimating the models of the preceding section. In fact, we reserved sample points over the five-year period, 1973–77, for use in forecasting analyses of our models. Since these are years for which *observed* values of the predetermined (exogenous and lagged dependent) variables in our estimated equations are available, forecasts based on these values customarily are referred to as *ex post fore-*

casts. If the models that we have estimated actually reflect stable causal mechanisms and are not mere exercises in curve fitting, ex post forecasts based on our models should be relatively accurate. In the absence of true structural change (which can never be completely discounted), ex post forecasts are one more means of determining the impact of many possible sources of measurement, specification, and estimation error on the results that we have obtained.

For each of the five years, 1973-77, table 4 presents information by which to evaluate the ex post forecasting performance of our final estimated versions of our models. Lines 1, 4, and 7 of this table report the "spliced UCR/SI-73 estimates" of the robbery, burglary, and automobile theft rates that we obtained by methods described earlier in this paper. Forecasts of these rates (reconverted from natural log units into rates per 100,000 population) based on equations (3), (5), and (7) are given in lines 2, 5, and 8. The corresponding annual forecast errors (spliced estimates minus forecast values) are noted in lines 3, 6, and 9 of the table. Thus, a positive forecast error indicates that a crime rate forecast falls short of the spliced estimate of the reported crime rate, while a negative error indicates that the forecast value exceeds the estimated rate.

One way to evaluate the comparisons in table 4 is to examine them on a year-by-year basis. For instance, both the forecasts based on our robbery rate equation (eq. [3]) and those based on our final burglary rate equation (eq. [5]) are too high for 1973, 1976, and 1977, virtually on target for 1975,

TABLE 4
EX POST FORECAST COMPARISONS FOR ROBBERY, BURGLARY, AND
AUTOMOBILE THEFT RATES, UNITED STATES, 1973-77

	1973	1974	1975	1976	1977	RMSE*	SEE*
Robbery Rate							
1. Spliced estimates	182.1	208.2	217.1	194.8	186.1
2. Forecasts, eq. (3)	196.9	199.4	217.1	229.7	216.5
3. Forecast errors	-14.8	8.8	.0	-34.9	-30.4	.1077	.0846
Burglary Rate							
4. Spliced estimates	1,205.7	1,418.0	1,505.0	1,419.7	1,391.6
5. Forecasts, eq. (5)	1,216.0	1,330.6	1,496.3	1,493.5	1,509.6
6. Forecast errors	-10.3	87.4	8.7	-73.8	-188.0	.0517	.0443
Automobile Theft Rate							
7. Spliced estimates	439.1	458.5	465.7	442.5	444.0
8. Forecasts, eq. (7)	474.0	487.2	485.6	512.6	529.8
9. Forecast errors	-34.9	-28.7	-19.9	-70.1	-85.8	.1132	.0411

NOTE.—RMSE = root mean square error.

* These statistics are reported in natural log units. All other numbers in the table are given in conventional rates per 100,000 population.

and too low for 1974. For all five years, however, it should be emphasized that the robbery forecast errors are less than twice the standard errors of the forecast equation; for the burglary rate, this statement is true for all the forecast years except 1977, for which the forecast error is about three times the standard error of estimate.¹⁸ Both because of the length of the forecast period and because the observed values of the robbery and burglary rates do not exhibit "simple linear trend" behavior over the period, it must be concluded that this is a highly accurate record of *ex post* forecasting. In contrast, in addition to being in error by about four times the standard error of estimate of the forecasts equation for 1976 and 1977, the forecasts of the automobile theft rate are consistently too high throughout the five-year period. In part, this may be due to the introduction of improved automobile ignition security systems by manufacturers in the early 1970s. These improvements in automobile security make thefts more difficult for amateurs and are not taken into account in the causal mechanisms that we have specified in equation (7).

Note also that our forecasts for all three property crime rates are too high for 1976 and 1977. The consistency of this pattern across crimes suggests that it may be due to a period-specific cause unique to the two most recent years of our forecast period. An obvious candidate is the pattern of severe winters that began in the winter of 1976-77. The validity of this interpretation is very difficult to assess, but a reduction in estimated property crime rates on the order of 10% due to weather-related curtailments in exposure to risk of crime in transit and in other nonresidential locations is not implausible. Since our residential population density ratio is based on components that are not very sensitive to such transient alterations in behavior patterns, these period-specific changes will not be reflected in our forecasts.

Although an examination of forecast errors for single years may suggest important period-specific factors that are not taken into consideration by our models, a more crucial question is whether the overall pattern of errors throughout the forecast period is comparable with the pattern of errors during the estimation period. One means of assessing this is to compare the root mean square errors (RMSEs) for the forecast period with the standard errors of estimate (SEEs) of the estimation period. The RMSEs are computed by taking square roots of the means of the squared forecast errors. Table 4 reports a RMSE for each equation. If the forecast-period errors are drawn from the same probability distribution as the estimation-period errors, then the RMSE value for an equation should be comparable in magnitude with its SEE (Christ 1966, pp. 621-26). For ease of comparison, the SEE of each forecast equation is also given in table 4. Note that the

¹⁸ Note that this statement is made on the basis of the standard errors of estimate of the equations, whereas a more proper statistical statement would be based on the forecast variance (see, e.g., Johnston 1972, pp. 152-55). However, since the forecast variance always is greater than or equal to the estimate variance, a forecast error that passes a test based on the latter will certainly do so for a test based on the former.

RMSEs for the robbery and burglary rate equations are about the same order of magnitude as their SEEs, whereas the RMSE for the automobile theft equation is about three times as large as its SEE. This suggests, in conformity with our period-specific analysis, that our robbery and burglary equations continue to remain valid in the forecast period, while the automobile theft equation may need to be respecified and/or reestimated.

To follow up on this suggestion, we introduce a reestimated automobile theft equation in table 5. In lieu of an annual time series on the proportion of the extant automobile stock that incorporates the improved ignition security systems introduced in the early 1970s, we introduce in equation (8) of table 5 a dummy variable constant term that is zero through 1970 and one thereafter. This specification is based on the fact that the improved systems became mandatory beginning with the 1971 model year (Office of Federal Register 1970, p. 231). We expect this dummy variable to adjust downward the equilibrium position toward which our automobile theft equation is tending and, possibly, to decrease the rate of adjustment of the equation to its equilibrium level. These expectations are borne out by the negative and statistically significant coefficient of the dummy variable in equation (8) and by the estimated decrease in the adjustment coefficient

TABLE 5
ESTIMATED AUTOMOBILE THEFT RATE EQUATION,
UNITED STATES, 1947-77

Predetermined Variables	Estimated Equation (8)
Constant.....	3.6491 (4.5395)*
Dummy variable (1971-77 = 1, 0 otherwise) ..	-.0995 (2.9973)*
ln auto theft rate lagged ($t-1$).....	.6367 (7.8754)*
ln residential population density ratio (t).....	1.4599 (7.3053)*
ln unemployment rate (t).....	-.1609 (4.3027)*
ln proportion aged 15-24 (t).....	.2110 (1.3684)
Summary Statistics	
\bar{R}^29929
SEE.....	.0446
D-W.....	1.7261 ^a
h8539 ^a
df.....	25

NOTE.—See table 1.

^a Fail to reject the null hypothesis of zero autocorrelation of disturbances at $\alpha = .01$.

* Significantly nonzero at $\alpha = .10$, one-tailed test.

from about 0.51 (1.0 — 0.49) in equation (7) of table 3 to about 0.36 (1.0 — 0.64) in equation (8) in table 5. The remaining coefficients in equation (8) are similar in magnitude to those of corresponding variables in equation (7), indicating that the estimated structure is robust under introduction of the dummy variable. The summary statistics for equation (8) show that the consistent overprediction of automobile theft rates for 1973–77 by equation (7) is sufficiently well treated by equation (8), so that its disturbances are not autocorrelated and its overall performance is good. In brief, we conclude that the reestimated automobile theft equation given in table 5, by taking into account recent technological changes in the automobile security systems, provides an improved basis for making forecasts.

DISCUSSION WITH EX ANTE EQUILIBRIUM FORECASTS FOR THE MID-1980s

The statistical estimation and forecasting analyses of the two preceding sections have been guided by both theoretical and practical goals. On the one hand, we have sought to evaluate our theory of how changes in criminal opportunity affect Index Crime property crime rates. On the other hand, we have attempted to assess whether this theory leads to models that provide reasonably accurate forecasts of Index Crime property crime rates in periods beyond which they have been estimated. We conclude on the basis of the preceding analyses that there is substantial empirical support for the two corollaries derived from our main theorem.

In addition to the direct tests of the ability of the main variables of our theory to explain trends in index property crime rates, it should be noted that, under our attempts to hold other things equal, we corroborated the importance of two additional variables long considered relevant to the explanation of property crime rates by criminologists (see, e.g., Gould 1969; Mansfield et al. 1974; Nettler 1978)—age structure and the availability of suitable property targets. Regarding the former, we found, as have many other investigators, that the proportion of the population in the teenage and young adult years (ages 15–24 in our analyses) is consistently associated with levels of reported robbery, burglary, and automobile theft rates. Similarly, we found a positive effect of nonautomobile consumer durables expenditures—our index of property target availability—on the burglary rate. Although a similar control for the number of automobiles per capita available for theft did not prove to be statistically significant, we noted that the temporal variation is largely captured by the residential density ratio. Therefore, the latter index acts as an effective control for the availability of automobiles for theft.

The results of our ex post forecasting analyses of the models specified on the basis of these theoretical considerations were somewhat mixed. For the

robbery and burglary rates, the forecasts were reasonably good over the entire forecasting period, 1973-77, and quite good for the years 1973-75. We concluded that the relatively poorer forecasts for 1976 and 1977 may have been due to severe winter weather patterns unique to recent years. On the other hand, in addition to being substantially in error for 1976 and 1977, our automobile theft rate forecasts were consistently too high throughout the forecast period. This led to the incorporation of the effects of recent technological improvements in automobile ignition security systems into a reestimated automobile theft equation.

One noteworthy characteristic of all of the estimated coefficient structures of equations (3)-(8) in tables 1-3 and 5 is that, in each case, the sum of the estimated coefficients exceeds 1.0. Since these are log-linear versions of multiplicative functions of the form of equation (1), this implies that an increase (decrease) of, say, $k\%$ in each of the predetermined variables in a given equation will produce an increase (decrease) of greater than $k\%$ in the dependent variable of the equation. In economic theory, this property of coefficients summing to a number greater than 1.0 for multiplicative equations of the form of equation (1) is known as "increasing returns to scale" in production (Henderson and Quandt 1971, p. 79). Thus, another way of stating the implication of this arithmetic fact is to say that it implies "increasing returns to scale" in the social forces that alter the criminal opportunity structure for Index Crime property crimes.

This property of increasing returns to scale can be vividly illustrated by examining the proportionate changes expected in the robbery, burglary, and automobile theft rates for a future year, such as 1985, with the corresponding proportionate changes in the exogenous variables of our forecasting equations. Table 6 reports the information necessary for such comparisons. Since most of the equations used to make the 1985 forecasts in table 6 are difference equations, the comparisons are given in terms of the implied equilibria of the last year of our ex post forecasting analyses (1977) versus the correspond-

TABLE 6
EQUILIBRIA FORECAST COMPARISONS FOR ROBBERY, BURGLARY,
AND AUTOMOBILE THEFT RATES, UNITED STATES,
1977 AND 1985

EQUATION USED TO FORECAST	EQUILIBRIA	
	1977 (Ex Post)	1985 (Ex Ante)
Robbery rate, (3).....	318.3	236.3*, 217.4†
Burglary rate, (5).....	1,509.6	1,798.2*, 1,821.4†
Automobile theft rate, (8)....	536.0	492.3*, 454.1†

* Assumes a 5% unemployment rate.

† Assumes a 6% unemployment rate.

ing implied equilibria for 1985.¹⁹ The latter are referred to as *ex ante* forecasts because they are based on projected values of the exogenous variables of the equations in contrast to the observed values used in the *ex post* forecasts.²⁰ Specifically, the forecasts in table 6 are based on (1) a projected residential population density ratio in 1985 of 0.6374 versus a 1977 observed value of 0.6615 (a 4% decline), (2) a projected proportion of the population aged 15–24 in 1985 of 0.1644 versus a 1977 observed value of 0.1902 (a 14% decline), (3) assumed consumer expenditures for nonautomobile durable goods of \$113.0 billion in 1983 versus \$43.4 billion observed in 1975—both in 1967 dollars (a 62% increase), and (4) assumed unemployment rates of 5% and 6% for 1985 versus 7% for 1977 (14% and 28% declines, respectively). Note that these modest proportionate changes in the exogenous variables of equations (3), (5), and (8) produce *ex ante* forecast equilibria for 1985 that are 26%–32% *lower* than the 1977 *ex post* value for robbery, 19%–21% *higher* for burglary, and 8%–15% *lower* for automobile theft.

We conclude with several comments and cautions about the interpretation of these *ex ante* forecasts. First, the fact that they are equilibrium forecasts means that they may not be very closely realized in 1985. For substantial accuracy, it would be necessary for the levels of the exogenous variables to have been maintained at the values assumed in making the forecasts for three or four years prior to 1985. This would allow the adjustment mechanisms in the difference equations to work themselves out, for the most part, by 1985. The only exception is, of course, the burglary forecast, which, since it is based on an algebraic rather than a difference equation, does not require an adjustment period longer than one year. On the whole, the forecast equilibria probably characterize rates for the mid-1980s in general better than those for the exact year 1985. Nevertheless, even with this cautionary note, the directions of forecasted changes in the rates for the mid-1980s as compared with the late-1970s can be inferred from a comparison of the equilibria given in table 6. As noted, these comparisons imply substantial decreases in robbery and automobile theft in contrast to a sizable increase in burglary.

Regarding the latter, it is clear from the estimated structure of equation (5) in table 2 that the major exogenous change forcing the forecast increase

¹⁹ For first-order log-linear stochastic difference equations of the form of eq. (2), the implied equilibria, conditional on specified values of the exogenous variables, are given by computing $\hat{\alpha}^*/1 - \hat{\beta}$, where $\hat{\alpha}^*$ is defined as the sum of estimated constant term plus the natural logarithm of each exogenous variable multiplied by its estimated coefficient, and $\hat{\beta}$ is the estimated value of the coefficient of the lagged dependent variable.

²⁰ The 1985 projected levels of the components of the residential population density ratio and of the proportion of the population aged 15–24 were obtained from 1985 census projections of these components and proportion, as given in the sources cited in the Appendix. The 1985 forecast of consumer expenditures on nonautomobile durable goods is based on forecasts obtained from the National Planning Association, Washington, D.C.

in burglary for the mid-1980s is the consumer durable expenditures index of property target availability. In fact, if a 1985 burglary rate equilibrium forecast is based on a reestimation of equation (4) with the consumer durable expenditures variable left out of the equation (in which case, the unemployment rate also is statistically insignificant and therefore deleted, but the lagged dependent variable is retained), the 1985 value *drops* about 24% from 1977 (1,675.0 for 1977; 1,275.9 for 1985). This result is much more in line with those for robbery and automobile theft. Thus, if one maintains that an increase in the availability of nonautomobile durable property targets has a significant association with the burglary rate, as was found in our estimation and ex post forecasting analyses, one is led to expect an increase in the burglary rate for the 1980s. However, the burglary rate has tended historically to move in the same direction as the robbery and automobile theft rates. Therefore, if one accepts the premise that this association will continue into the next decade, one is led to conclude that the major social forces affecting changes in the burglary rate should be the same as those affecting robbery and automobile theft. From this, it follows that a decrease is to be expected in all three rates.

We will not try to discriminate between these alternatives here. Our objective is merely to illustrate the conditionality of the forecasts in table 6 on the structure of the equations used to generate them. These forecasts, of course, are conditional on the values of the exogenous variables on which they are based and on the usual *ceteris paribus* assumption of ex ante forecasting analyses. As with all ex ante forecasts, our efforts are based on structures estimated on the basis of past behavior and assume that the technology, social organization, and behavior patterns that have governed the reactions of Americans to criminal risk in the past will apply in the future. For example, our forecasts obviously assume that there will be no major technological advances in domestic security systems during the next decade, no major reorganizations of police or citizen behavior patterns vis-à-vis guardianship or exposure to risk of property crime, no major increase in the effectiveness of the criminal justice system in controlling property crime, etc.²¹ They also assume no major alterations in social

²¹ Although a systematic analysis of the ability of the criminal justice system to control index property crime rates is beyond the scope of this paper, a few observations can be made. First, various recent studies by social scientists and policy analysts have concluded that incapacitation of offenders, especially repeat offenders, may reduce crime rates by removing such individuals from the general population, apart from whatever deterrence effects, if any, may also be created (for a review of these studies, see Cohen [1978]). In the context of the time-series models constructed above, however, our preliminary analyses reveal a statistically significant negative relationship of trends in imprisonment to trends in property crime rates only for robbery. Since robbery involves an element of violence and violent crime offenders are more likely to be imprisoned than property crime offenders, this finding is not unexpected. But, although this negative relationship performs well statistically during our estimation period (1947-72), it leads to consistent, gross under-

organization or individual behavior patterns in reaction to the forecasts themselves!

APPENDIX

Sources for Time-Series Data on Exogenous Variables

Population aged 15-24.—*Current Population Reports*, P-25, nos. 311, 519, 643, 721, U.S. Department of Commerce.

Female labor force participation rate, husband present.—*Historical Statistics of the United States*, 1975, U.S. Bureau of the Census; *Statistical Abstract of the United States*, 1972-77, U.S. Bureau of the Census; *Special Labor Force Reports*, U.S. Bureau of Labor Statistics.

Unemployment rate.—*Historical Statistics of the United States*, 1975, U.S. Bureau of the Census; *Manpower Report of the President*, 1977, U.S. Bureau of Labor Statistics; *Economic Report of the President*, 1977-78, U.S. Government Printing Office.

Autos per capita.—*Historical Statistics of the United States*, 1975, U.S. Bureau of the Census; *Statistical Abstract of the United States*, 1972-77, U.S. Bureau of the Census; *Motor Vehicle Facts and Figures*, 1978, Motor Vehicle Manufacturers Association of America, Inc.; *Current Population Reports*, P-25, nos. 311, 519, 643, 721, U.S. Department of Commerce.

Residential population density ratio.—Abbot L. Ferriss, *Indicators of Change in the American Family* (New York: Russell Sage Foundation, 1970); *Historical Statistics of the United States*, 1975, U.S. Bureau of the Census; *Statistical Abstract of the United States*, 1970-77, U.S. Bureau of the Census; *Current Population Reports*, P-20, no. 313, U.S. Department of Commerce; *Employment and Training Report of the President*, 1978, U.S. Government Printing Office.

Nonautomobile consumer expenditures, constant 1958 dollars.—Hickman and Coen Data Bank (data bank used by Bert G. Hickman and Robert M. Coen in *An Annual Growth Model of the U.S. Economy* [Amsterdam: North-Holland, 1976]).

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predictions of estimated robbery rates during our ex post forecasting period (1973-77). This suggests temporal instability in the relationship; this very instability should be one of the subjects of future research.

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Metropolitan Morphology and Population Mobility: The Theory of Ecological Expansion Reexamined¹

David F. Sly

Florida State University, Tallahassee

Jeffrey Tayman

State of Washington

The theory of ecological expansion is examined and propositions are developed which suggest that, at advanced stages of metropolitanization, population loss resulting from migration is a function of the decentralization of nonresidential activities from cores to rings coupled with the increased importance of rings as residential centers and the incursion of the residential function into areas beyond the boundaries of the metropolitan community. The changing structure of rings suggests that they are emerging as the hubs of metropolitan activities and that they are serving an increasingly central role, integrating cores and areas beyond the boundaries of metropolitan communities as the residential function invades the latter. Although hypotheses are developed to test each of the propositions offered, and although our analysis lends support to the revisions of the theory, the analysis is greatly constrained by the lack of fit between the metropolitan area concept and the census demarcated Standard Metropolitan Statistical Areas.

From 1970 through 1974 a number of metropolitan areas in the United States lost population as a result of migration; that is, they had net migration losses. Although this trend had already emerged during the 1960s, when 40% of the nation's metropolitan areas had net migration losses (U.S. Bureau of the Census 1971), the phenomenon did not receive a great deal of attention at the time because these losses were offset by population increases resulting from natural change in nearly all areas. During the 1970s, however, rates of natural increase in the metropolitan population declined, and a number of metropolitan areas are now actually declining in population. Migration is making a substantial contribution to the decline of the metropolitan and the increase of the nonmetropolitan population. The Census Bureau estimates that from March 1970 to March

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1974 migrants from metropolitan to nonmetropolitan areas outnumbered those from nonmetropolitan to metropolitan areas by some 1.8 million persons (U.S. Bureau of the Census 1976).² Recently completed studies of the urban agglomeration processes in some of the world's most industrialized countries indicate that this phenomenon is not unique to the U.S. setting (Goldstein and Sly 1977).

To date social scientists (and the popular media) have been more concerned with describing and documenting (1) the population redistribution patterns of this phenomenon (Beale 1975, 1976; Wardwell 1976), (2) the characteristics of metropolitan to nonmetropolitan migrants (Kirschenbaum 1971; Fredrickson 1974; Tucker 1976; DeJong and Humphrey 1977), and (3) the motives for nonmetropolitan residence (Dillman and Dobash 1972; Fuguitt and Zuiches 1975) than with the structural causes and consequences of the mobility for the areas affected. The major purpose of this paper is to examine variations in the rates of net migration among the most industrialized metropolitan areas in the United States during the period 1970-74, giving particular attention to the structural conditions existing in these areas in 1970. Specifically, the theory of ecological expansion is reexamined and a number of "implied" propositions relating structural changes in the agglomeration process to patterns of population change resulting from migration are identified and tested. The theory is tested in two respects. First, we ask whether it produces any propositions which might help to explain contemporary patterns of population redistribution; that is, is the theory useful in helping to generate explanations of observable phenomena? Second, we develop a series of hypotheses related to the implied propositions and then proceed to test the hypotheses. The tests are inevitably, and markedly, restricted by (1) their bivariate nature, (2) the methodological constraints resulting from having to work with politically demarcated units of SMSAs, and (3) the definition of migration employed.

It is important to note that the two latter constraints are closely interrelated. The county units forming SMSAs may demarcate accurately the functional boundaries of the metropolitan areas, but in most cases they either under- or overbound the functional unit. Similarly, the technical definition of a migrant employed by the Bureau of the Census requires that a person cross a county border before being counted. Thus, it is pos-

² Despite this net migration loss to formally defined metropolitan areas (SMSAs), it is important to bear in mind that it is not at all clear that this represents a true loss to the functional unit: the vast majority of the net migration from metropolitan areas is being absorbed in the nonmetropolitan counties contiguous to metropolitan areas (Beale 1975). Moreover, Berry and Dahmann (1977) have recently shown that these are the fastest growing nonmetropolitan counties and that the proportion of their residents commuting to metropolitan areas for work tends to be a substantial one for nonmetropolitan areas.

sible for SMSAs to have net migration losses that do not portray accurately the pattern of population loss (or the lack of it) from the functional unit. Although the "migration" we discuss below may be more rightly referred to as "residential mobility," we have employed the terms "migration" and "population loss resulting from migration" because, from a technical perspective, they reflect more accurately the limitations of the data and Census Bureau concepts. Moreover, there is no way to determine the origins and destinations or, for that matter, the actual number of moves which contribute to the net migration effects reported for the areas studied.

THE THEORY OF ECOLOGICAL EXPANSION

Human ecology's interest in the territorial and temporal distribution and redistribution of population dates from the seminal works of Burgess (1925), McKenzie (1933), and Park (1936) and their concepts of fluidity and mobility. Although McKenzie (1933) gave a prominent role to migration in the development of the metropolitan community and regional specialization, his primary focus was on migration's influence on these things rather than on their effects on migration. In more recent years a number of studies showing varying degrees of departure from this tradition (Biggar and Biasioli 1978; Frey 1978; Olsen and Guest 1977; Hinze 1977; Farley 1976; Frisbie and Poston 1975; Berry and Cohen 1973; Sly 1972; Karp and Kelly 1971) have laid the groundwork for a more systematic consideration of the role of metropolitan community structure in influencing patterns of migration. While a number of alternative theoretical models are presented by these researchers, most appear to have some roots in the theory of ecological expansion or the ecological tradition. For instance, some researchers (Hinze 1977; Karp and Kelly 1971) have utilized an equilibrium framework while others have examined migration within the general outline of the population-organization-environment-technology (POET) complex (Sly and Tayman 1977; Frisbie and Poston 1975; Sly 1972), and some have focused on the redistributive mechanisms suggested by the theory of expansion (Biggar and Biasioli 1978). Although it is impossible to review all of these studies here, or even to give a full statement of the theory of expansion, we can sketch the broad outline of the theory highlighting what we feel are some of its important implications for migration.

The theory of ecological expansion is concerned with the process of metropolitanization and the changing form and structure of metropolitan communities. According to this theory, metropolitan communities develop as functionally integrated units with two interdependent areas largely as a result of centrifugal and centripetal movements of people and activities

(McKenzie 1933; Park 1936). One of these areas is an inner core whose traditional primary functions are to coordinate and integrate the two areas into one functioning unit. The second area is the outer ring, which comes increasingly under the dominance of the core as the ecological distance between previously independent communities and the core is reduced by technological innovations related to transportation and communication (Burgess 1925; McKenzie 1926; Ogburn 1946; Hawley 1950, pp. 145–75). As expansion of the metropolitan community continues, formerly independent and quasi-independent communities lose many of their specialized functions and services to the inner core where the full advantages of external economies offered by location can be had. With the centralization of economic activity and jobs in the inner core, the outer area takes on increasingly the character of residential centers whose primary function is to house workers for the inner core; this, however, is seldom the exclusive function of suburbs (Schnore 1956; Berry and Cohen 1973; Palen 1975).

Although these processes produce identifiable functionally segregated areas, it is important to note that the boundaries separating them are in a constant state of change and that, although one area may be dominated by a particular function, other functions are not excluded (Zimmer 1975). Thus, for instance, although the primary function of the inner core is the integration and coordination of the entire area, it also serves, in varying degrees, a residential function, a wholesale and retail function, and a manufacturing function among others (Johnson 1957; Hawley 1971; Kasarda 1972). Indeed, at the earliest stages of metropolitan development all the functions of the community are located in one relatively compact area; through the processes of invasion and succession they move outward at the same time that formerly independent communities are brought within the dominance of the metropolitan area (Gras 1922). Although these communities lose much of their independent character and have many of their functions taken over by the larger unit, economic forces also come into play which prevent these areas from playing an exclusively residential role.

Initially, high land prices, the sheer lack of space, and communication and transportation links facilitate the expansion of the original functions of coordination and integration to wider and wider areas as the size of these functional sectors increases with both the areal and the demographic increase of the metropolitan area (Berry and Cohen 1973; Borchert 1972; Kasarda 1976). Thus, with development and growth of the metropolitan area many of the functions once concentrated in the core begin to invade the ring. At advanced stages of metropolitanization the lines separating ring from core and the functional segregation of the two become less and less pronounced. That is, the rings increasingly take on many of the functional characteristics of the core and the core takes on many of the func-

tional characteristics of the ring—within each there emerges a functional balance.

Moreover, as these processes continue, the outer boundaries of metropolitan areas become ever more difficult to determine. Commuters who once had to live within reach of the core may now find their jobs relocated within the ring making it increasingly possible for the residential function to disperse to areas farther from the core (Elazar 1973; Guest 1975). With the growth and decentralization of additional functions, expansion continues and it becomes increasingly possible for such residential areas to develop more functional links to the ring than to the core. This process aids further in the nonresidential development of the ring and provides additional impetus for it to take on characteristics formerly belonging to the core.

While the theory of expansion says nothing explicit about population change in metropolitan areas, let alone the components of such change, some researchers (Hawley and Zimmer 1970; Guest 1971; Schnore 1972) have demonstrated important links between the process of expansion and the changing concentration and composition of population in cores and rings.³ A number of the structural changes outlined in the theory, however, have implications for whether or not areas will grow as a result of migration.⁴ For instance, the theory seems to imply the proposition that, as the nonresidential function of the ring portion of metropolitan areas increases, the rate of population loss due to migration will increase when large portions of societal or regional populations are already located in metropolitan areas. That is, as the nonresidential functions increasingly expand into the rings, economic factors (e.g., speculative land prices) reduce the space available for residential use and the decentralization of nonresidential functions permits and actually encourages deconcentration of residential areas. Thus we expect:

1. As the percentage of SMSA jobs in the ring increases, the rate of population loss to the SMSA resulting from migration will increase, or the rate of population increase resulting from migration in the metropolitan area will be inversely related to the percentage of the area's jobs located in the ring.
2. As the percentage of SMSA population living in the ring increases,

³ From a theoretical perspective, these structures and processes are more germane to the issues discussed than is chronological age. This is not to deny the potentially important role of age in the ecological analysis of metropolitan areas, but it is intended to emphasize the fact that age (in and of itself) has little theoretical import. The important elements are the changing structures and processes, and in turn, their implications for the functioning unit.

⁴ In this regard it is important to note that some studies have dealt with the composition of migration streams into cores and rings, but no one has dealt systematically with the effects of the interrelations between these and the components of change in whole metropolitan areas.

the rate of population loss to the SMSA resulting from migration will increase, or the rate of population increase resulting from migration will be inversely related to the percentage of the area's population residing in the ring.

A second proposition suggested by the theory of expansion relates population loss due to migration to particular types of nonresidential functional expansion. The expansion of the coordinating and integrating functional sector of metropolitan areas is an indicator of broader metropolitan development in general and of nonresidential expansion in particular. That is, the decentralization of coordinating and integrating functions lags behind that of other nonresidential functions because decentralization of the former functions is dependent on the functional mix which emerges in ring areas with expansion (Kasarda 1972). Thus, as ring areas become functionally more diverse, the demand for the coordinating and integrating functions in rings increases as does the demand for services. At advanced stages of metropolitanization, population loss due to migration will be more directly a response to the decentralization of coordinating and integrating and service functions, largely because the earlier invasion of the ring by other nonresidential functions has led to competition for space between the newly decentralized functions and the residential function. That is, the population loss resulting from migration represents an expansion of the residential function. Thus, we should expect:

3. The association between the percentage of coordinating and integrating jobs located in the ring portion of SMSAs is directly related to the extent of population loss due to migration, and this association is stronger than the more general association between the percentage of all jobs located in the ring and population loss due to migration.

4. The association between the percentage of service jobs located in the ring and population loss due to migration will be positive and stronger than the association between the percentage of other specific types of jobs located in the ring and population loss due to migration.

Implicit in each of the foregoing propositions is the idea that the rings of metropolitan areas undergo a structural transformation as nonresidential functions invade them and compete with the residential function. Under the pressure of these developments, rings increasingly acquire the same functional characteristics as the cores; the residential function once concentrated in the ring invades the areas beyond the immediate boundaries of metropolitan areas. The ring and core areas do not lose their residential functions, but these do become less and less dominant as structural characteristics. The deconcentration of other than nonresidential functions reduces the external economies of the new residential invasion. That is, decentralization allows new residential areas to be farther from the core and still have the same degree of access to nonresidential func-

tions and services once available only in the core. Indeed, the emergence of new residential areas beyond the boundaries of metropolitan areas; plus the decentralization of nonresidential functions, makes the ring portion of metropolitan areas increasingly the hub of metropolitan activity. The lag in the decentralization of coordinating and integrating functions to the ring may also suggest that the invasion of new residential areas is disproportionately contributed to by the population engaged in these types of activities. If the areas beyond the boundaries of metropolitan areas are emerging as centers of the residential function, we should expect:

5. As the percentage of jobs located in the ring increases, the flow of workers from outside the SMSA to the ring increases.

6. As the percentage of coordinating and integrating functions in the ring increases, the flow of workers engaged in these functions from outside the SMSA to the ring will increase. This association will be stronger than that between all jobs and the flow of workers in general from outside the SMSA to the ring.

Similarly, if the ring portions of metropolitan areas do emerge as the hub of activity at advanced stages of metropolitanization and if this is the result of deconcentration and expansion, we should expect that there is a propensity for metropolitan activity to orient itself increasingly toward the ring portions of metropolitan areas. That is,

7. As the percentage of jobs in the ring increases, the flow of workers from the core to the ring will increase.

8. As the percentage of jobs in the ring increases, the percentage of lateral commuters within the ring will increase.

9. As the percentage of jobs in the ring increases, the percentage of commuters from the ring to the central city will decrease.

10. As the percentage of jobs in the ring increases, the percentage of lateral commuters within the central city will decrease.

11. Each of the relationships in hypotheses 7–10 will be stronger when only the coordinating and integrating functions are considered.

Finally, if there are functional ties between metropolitan areas and the emerging residential centers beyond their boundaries, we would expect these functional ties to have an impact on metropolitan population change resulting from migration. We can expect that new residential centers produced by the invasion of areas beyond the boundaries of metropolitan communities will come under the dominance of the ring as they send their residents to places of employment there. Such centers will affect population change in metropolitan communities not only directly, by drawing population from the metropolitan area, but also indirectly, by providing an alternative place of residence for potential movers into the vicinity. Thus, we would expect that the stronger the functional link between metropolitan areas and contiguous areas, the greater the likelihood of residential

expansion resulting in population loss due to migration. Moreover, since these functional ties and population loss resulting from migration have both been postulated to be products of the expansion process and of particular levels of expansion, we would expect controlling for the impact of expansion to reduce, and conceivably even reverse, the relationship between metropolitan areas and the areas beyond their boundaries. That is, in the absence of expansion the functional link between metropolitan areas and the areas beyond their boundaries can be expected to have a positive impact on population change resulting from migration into the former since in this case the latter are not competing or the level of competition with the former for the residential function is minimal. Thus,

12. The greater the proportion of workers from outside the metropolitan area who commute to the ring for employment, the greater the population loss or the less the gain resulting from migration.

13. The relationship above will reduce toward zero or reverse when the extent of expansion of the residential and nonresidential functions of the metropolitan area is taken into account.

DATA AND METHODS OF ANALYSIS

The Sample

Preliminary tests of the 13 hypotheses are possible only if we accept census demarcated areal components of Standard Metropolitan Statistical Areas. The acceptance of those components, however, presents a number of problems, the confounding effects of which cannot be measured. The most serious is the incompatibility between the politically bounded units of the census and the functionally demarcated conceptual units. In general the core conceptual component is underbounded by central city political lines while the outer boundaries of rings (SMSA boundaries) are overbounded by county lines (Goldstein and Sly 1974). In specific instances, some researchers have even argued that outer boundaries of metropolitan communities are underbounded by SMSA census criteria. Although there is no way of overcoming these boundary problems, a number of researchers established some time ago that they could be minimized by selecting the SMSAs with the largest central cities (Schnore 1959; Duncan et al. 1960; Feldt 1965; Berry, Goheen, and Goldstein 1968).

Taking note of this and remembering that we are concerned primarily with core expansion, we decided that the most conservative set of SMSAs we could study would be those with the largest central cities. Nevertheless, it is particularly important to note that this decision does not eliminate the boundary problem and that there is really no way of quantifying the extent to which the decision has helped to reduce the problem. The

data analyzed below are for all metropolitan areas which had a central city containing at least 250,000 inhabitants in 1970.

Of the 50 SMSAs which meet this criterion, 26 had net migration declines from 1970 to 1974. The 26 are only 23% of the 111 SMSAs losing population as a result of migration, but they account for 80% of the net out-migration of 2.7 million persons from the 111 SMSAs. Of the 50 SMSAs studied here, 17 are located in the South, 15 in the North Central region, and the remainder in the Northeast and West; at least one "in-" and three "out-" migration SMSAs from each region are included.⁵ Although the boundary problems have been minimized, any observations must still be qualified by their potential influence.

Indicators

In general it is possible to classify all of the variables mentioned in the hypotheses listed above into three major types: demographic variables, indicators of expansion, and functional linkages. The only *demographic variable* that we are interested in is the net migration rate for the 50 metropolitan areas for the period 1970-74. The data from which these rates were calculated are estimates of net migration produced by the U.S. Bureau of the Census (1976) using the vital statistics approach; the calculated rates represent change due to migration expressed as a percentage of the 1970 SMSA population. Ideally, we would like to have data on the gross (in and out) flows of population for metropolitan areas and their contiguous counties by place of origin and destination, but these data are not available.

A second set of variables employed in the analysis consists of *indicators of expansion*. All of them employ statistics for 1970. The indicator of residential expansion, used in a number of previous studies (Cuzzort 1955; Kitagawa and Bogue 1955; Hawley 1956), is merely the percentage of the total SMSA population residing in the ring (U.S. Bureau of the Census 1971, pp. 180-86). We have also included six indicators of functional expansion designed to tap differences in the extent of territorial distribution of sustenance niches between cores and rings of the SMSAs. The most general of these measures is an indicator of the distribution of all SMSA jobs between the component parts of the SMSA, the percentage of SMSA jobs located in the ring of each SMSA. This percentage was calculated by summing the total number of persons actually employed in manufacturing,

⁵ Region and size have been employed frequently in the past as proxies for age and a number of researchers have established the empirical basis for this (Schnore 1959, 1962). The fact that there are both in-migration and out-migration SMSAs in each region within this size class strongly suggests that structural differences and not age should be focused upon. This is made clear in the analysis reported below in n. 7.

wholesale, retail, and selected service establishments within each component part of the SMSA, and dividing the number of ring jobs by the total number of jobs (U.S. Bureau of the Census 1974*a*, 1974*b*, 1974*c*, and 1974*d*). These same data disaggregated to the four specific types of activities are employed as measures of different dimensions of functional expansion. Finally, because Kasarda (1972) has emphasized the importance of coordinating and integrating functions in the maintenance of core dominance, the percentage of each SMSA's coordinating and integrating functions located in the ring is our final measure of expansion. In making these estimates we followed the procedures used by Kasarda (1972), employing "journey to work" data.

The final set of indicators employed is designed to tap *functional linkages* between component parts of SMSAs and rings and areas beyond the SMSA boundaries. These data (again pertaining to 1970) are useful because a number of studies have documented the important role commuting to work plays in depicting the spatial patterns of SMSAs (Yu 1972; Guest 1975; Zuiches and Fulton 1976), and because other studies have related journey to work and migration patterns (Goldstein and Mayer 1964; Termote 1975). Five general commuting flows depicting the relative volume of persons residing in one component part of the SMSA and working in the other part or the same part or residing outside the SMSA and working in the ring have been operationalized as the total number of workers residing in the area divided into the specific flow except for the last flow, for which the base population was defined as the total number of workers living outside the SMSA and working inside it. The specific flow for workers in coordinating and integrating activities residing outside the areas and working in the ring was obtained using the latter procedure (U.S. Bureau of the Census 1972, 1973).⁶

Finally measures of significance have been reported for all the statistical tests of hypotheses. In essence we are dealing with a subpopulation and not a random sample. Several statisticians have argued for the use of significance tests in this situation because random error may result from factors other than sampling variation (Selvin 1957) and because differences among subgroups of a population can be due to chance and not the classification scheme (Blalock 1972).

DATA ANALYSIS

Hypotheses 1-4 and 12, presented above, are the simplest and most important for establishing the role of the expansion process in shaping patterns of metropolitan net migration. That is, each of them states that a

⁶ These measures are discussed in greater detail in Sly and Tayman (1977) and Termote (1975).

dimension of expansion should be more salient among metropolitan areas having net out-migration than among those having net in-migration. The rejection of these hypotheses would not necessarily cast doubt on the theory of ecological expansion, but it would indicate that our effort to broaden the theory to include expansion's consequences for patterns of metropolitan population change resulting from migration is ill founded.

To test these five hypotheses about the "net migration consequences" of expansion we calculated the means and standard deviations for each dimension of expansion separately for SMSAs having net population increase and decrease as a result of migration. These data along with the means and standard deviations for all SMSAs are presented in table 1. If the hypotheses are to gain support, we would expect those SMSAs with net out-migration during the period 1970-74 to be further along in the expansion process at the beginning of this period than were SMSAs still experiencing net in-migration.

At face value, the data lend considerable support to each of the hypotheses, and, according to the theory of expansion, no matter which indicator is selected the out-migration SMSAs were considerably more "expanded" than the in-migration SMSAs. For in-migration SMSAs, 34% of jobs and 45% of the resident population were in the rings. The compa-

TABLE 1
MEAN AND STANDARD DEVIATION OF NET MIGRATION AND
EIGHT INDICATORS OF EXPANSION FOR 50 SMSAS AND
FOR THOSE HAVING NET IN- AND OUT-MIGRATION

VARIABLE	ALL CASES		NET IN-MIGRATION		NET OUT-MIGRATION		t-TEST SIGNIFI- CANCE LEVEL*
	Mean	SD	Mean	SD	Mean	SD	
NM1970-74†..	1.61	6.37	6.34	6.15	-2.75	1.83	.001
EMPR(%)‡...	.41	.18	.34	.18	.48	.15	.005
POPR(%)§....	.51	.18	.45	.19	.56	.15	.02
CIFR(%) 34	.14	.29	.15	.38	.11	.01
OSTOR(%)#...	.32	.16	.28	.17	.37	.13	.006
RETR(%)**...	.45	.18	.40	.18	.53	.16	.002
WHOR(%)††...	.38	.18	.25	.16	.40	.16	.005
MANR(%)††...	.44	.19	.37	.20	.51	.16	.01
SERR(%)§§...	.32	.16	.28	.17	.37	.13	.05

* Testing for difference between in and out.

† Net migration 1970-74 as a percentage of 1970 SMSA population.

‡ SMSA employment in the ring, 1970.

§ SMSA population in the ring, 1970.

|| SMSA coordinating and integrating functions in the ring, 1970.

Workers living outside the SMSA and working in the ring, 1970.

** SMSA retail employment in the ring, 1970.

†† SMSA wholesale employment in the ring, 1970.

‡‡ SMSA manufacturing employment in the ring, 1970.

§§ SMSA selected service employment in the ring, 1970.

rable figures for out-migration SMSAs were 48% and 56%, respectively. When jobs are disaggregated to specific types, we can see that although the general patterns hold for each type, the extent of expansion varies considerably, with retail and manufacturing jobs decentralized to a considerably higher degree than wholesale and service jobs. Similarly, although the organizing and integrating jobs of out-migration SMSAs are more expanded than those in the in-migration SMSAs, they are considerably less expanded than all other types of specific jobs except for service. Finally, out-migration SMSAs also have more commuters from outside their boundaries to their rings than do in-migration SMSAs.

Three additional comments on these data are in order. First, the findings that organizing and integrating functions are not more expanded than all jobs and that service functions are not more expanded than other types of specific jobs should not be interpreted as contrary to hypotheses 3 and 4. These are analytically separate dimensions of expansion and can be expected to lag behind the other forms of nonresidential expansion. Expansion along these dimensions may not need to reach as high a level as that along the other dimensions before it has as great an impact on migration patterns, or even a greater one.

Second, although the mean of each expansion indicator is greater for the out- than the in-migration SMSAs, the reverse is true of the standard deviations in all cases except wholesale jobs, although the differences are not great. Even in the latter case, however, the larger mean for out-migration SMSAs and the equal standard deviations indicate that on each dimension of expansion the out-migration SMSAs show less variation as a group than do the in-migration SMSAs. The former are more homogeneous in that, as a group, they appear to have passed a crucial point in the expansion process and may even be moving in the direction of becoming a unique type of area.

Third, as a simple statistical test of the differences in expansion along each of the dimensions identified in table 1, we calculated *t*-tests which, in effect, ask whether, if a group of SMSAs had net out-migration during a given period, we can predict that these SMSAs were at a different stage of expansion from in-migration SMSAs at the beginning of the period. The results reported in the last column of table 1 indicate that the out-migration SMSAs are significantly different from the in-migration ones along each of the dimensions identified. That is, the *t*-tests of the differences between the in- and out-migration SMSAs for each expansion indicator show that in 1970 those areas having net losses from migration by 1974 were significantly more expanded than were the SMSAs having net population increases by 1974 as a result of migration.

The statistical analysis presented above only establishes the existence of some key relationships in our effort to broaden the theory of ecological

expansion. It does not test the strength of these relationships, nor does it constitute a good test of the hypotheses presented above since each hypothesis views both expansion and net change resulting from migration as continuous processes. In order to overcome these shortcomings and to test the remaining hypotheses, we reverted to correlation analysis using all 50 SMSAs. Seven of the hypotheses state a relationship between one of the indicators of expansion and net population change resulting from migration in such a way that we would expect the most expanded areas to have the highest rates of net migration loss. Thus, as the indicators of expansion increase in value, our measure of net migration should decrease in value. The correlation coefficients presented in table 2 indicate that this is, indeed, the case. Moreover, each of the nonresidential expansion indicators is more closely associated with net migration than is the residential indicator.⁷ Of particular importance for hypotheses 3 and 4 are the associations between net migration and (1) the percentage of coordinating and integrating functions in the ring (CIFR) and (2) the percentage of service jobs in the ring (SERR). These dimensions of expansion should be particularly important since the earlier analysis indicated that they were the least expanded. The percentage of coordinating and integrating

TABLE 2
ZERO-ORDER CORRELATION COEFFICIENTS
BETWEEN EXPANSION INDICATORS AND
NET MIGRATION FOR 50 SMSAS

Expansion Indicator	Correlation Coefficient	Level of Significance
EMPR(%).....	-.516	.001
POPR(%).....	-.295	.05
CIFR(%).....	-.713	.001
MANR(%).....	-.306	.02
RETR(%).....	-.314	.02
WHOR(%).....	-.297	.05
SERR(%).....	-.429	.001

⁷ For each of our most general indicators of expansion (the percentage of jobs in the ring and the percentage of population in the ring) separate analyses were carried out assessing their impact on net migration using partial correlations to remove the effects of age. With age measured as the number of years since the central city reached a population of 50,000 our results were mixed. Controlling for age had the effect of reducing moderately the association between the percentage of population in the ring and the rate of net migration (— .295 vs. — .234); however, the same control considerably strengthened the association between the percentage of jobs in the ring and the rate of net migration (— .516 vs. — .834). What this implies, of course, is that age is having a less important influence on the redistribution of jobs within the metropolitan area than on that of residences. One reason may be that newer metropolitan areas start with a greater degree of deconcentration than did older metropolitan areas. The association between chronological age and the processes of expansion may be important but requires far more attention and data than can be presented here.

jobs in the ring has a markedly stronger association with net migration than do all jobs, and the percentage of service jobs in the ring has a moderately stronger association with net migration than does any other type of specific activity (manufacturing, retail, or wholesale). In short, these data along with those presented in table 1 lend considerable support to hypotheses 1-4.

While these coefficients lend support to the hypotheses stating the impact of expansion on net population change resulting from migration, they do not indicate anything about the changing functional links between cities and rings; these issues are taken up in hypotheses 5-12. If these hypotheses are to gain support we should find that (a) as the percentage of jobs in the ring (%EMPR) increases, the flow of workers to the ring from outside the metropolitan area and from the city increases and that (b) there is a rise in the rate of lateral commuting within the ring while the flow of workers from the ring to the city and within the city decreases. If the crucial role of coordinating and integrating functions in the expansion process is to be confirmed, we should expect that each of the relationships above will be stronger when only commuters engaged in those functions are considered.

All of the correlation coefficients are in the predicted direction, although they tend to be of only moderate strength (table 3). In each case, the coefficient relating the expansion indicator with an indicator of functional linkage is stronger when only the coordinating and integrating functions are considered. Expansion is most closely associated with high flows of commuters from areas outside the metropolitan area into the ring, but it is important to bear in mind that this flow has been measured differ-

TABLE 3
CORRELATION COEFFICIENTS BETWEEN A
GENERAL EXPANSION INDICATOR AND
FIVE COMMUTING FLOWS FOR ALL COM-
MUTERS AND FOR THOSE WITH COORDI-
NATING AND INTEGRATING JOBS

EMPR(%)	COMMUTING RATE	
	All Jobs	Coordinating and Integrating
OTOR ^a816**	.896**
CTOR ^b458**	.752**
RTOR ^c510**	.602**
RTOC ^d	-.244*	-.303*
CTOC ^e	-.509**	-.785**

^a Outside to ring.

^b City to ring.

^c Within ring.

^d Ring to city.

^e Within city.

* Significant at .05 level.

** Significant at .001 level.

ently from the others and includes in the denominator only those from outside the area who work in either the city or the ring (rather than all workers irrespective of where they work as in the case of the other flows).

The weakest association occurs between expansion and the rate of commuting from rings to central cities, suggesting that expansion has the least impact on this functional linkage. This is particularly interesting given that the reverse flow (from city to ring) has nearly twice the strength of the former: expansion may have a greater impact on employment opportunities for the central city population than for the ring population. That is, the central cities' dominance over the metropolitan area may be lessening and the dominance of the ring may be increasing. In short, all of the coefficients in table 3 tend to support the general idea that expansion is making the populations of each component part of the SMSA more dependent on the ring than the core and that the latter is emerging as the dominant area of metropolitan activity, integrating areas beyond the metropolitan boundaries as well as the core.

As noted above, this suggests that areas beyond metropolitan boundaries are being functionally integrated into the metropolitan structure as emergent residential centers, both by attracting out-migrants from contiguous metropolitan areas and by providing convenient alternatives to the SMSA for migrants from elsewhere. The correlation between the proportion of workers from outside the metropolitan area who commute to the ring and the rate of metropolitan net migration is of sufficient magnitude ($-.282$) to support this notion. However, even more important is that this association is reversed when a partial association is calculated ($.232$) controlling for the extent of residential and job expansion. In the absence of expansion, commuting from outside the area into the ring may actually lead to a gain as a result of migration. That is, the more dominant the role of the core the more likely it is that the functional linkages between the metropolitan area and adjacent areas will draw population toward the metropolitan area.

CONCLUSIONS

The primary objective of this paper has been theoretical: we have attempted to specify some consequences of the theory of ecological expansion for population change in metropolitan areas. More specifically, our review of the theory suggests that several of the structural changes resulting from expansion imply that at advanced stages of metropolitanization population loss resulting from migration is likely to occur and/or that the greater the expansion, the less will be the positive impact of net migration on the population growth of metropolitan areas. The structural consequences which we have identified as particularly important in this

respect are (1) the decentralization of nonresidential functions from the cores of metropolitan areas to rings in combination with (2) the increased importance of rings as residential centers and the incursion of the residential function into areas outside the SMSA. In terms of the nonresidential functions, the theory implies that the decentralization of coordinating and integrating functions as well as service functions will lag behind that of other nonresidential functions and that by the time the former begin to decentralize, the latter, in combination with the residential function, will have resulted in rings taking on many of the characteristics more commonly associated with the core. The increasingly dominant position of rings over cores within metropolitan areas, coupled with the links established between rings and emerging residential areas beyond the boundaries of metropolitan communities, operates to offer residential alternatives to migrants who wish to live outside the metropolitan community but within commuting distance of ring activities. This alternative is available not merely to metropolitan residents but also to outsiders who may move into the vicinity of the metropolitan area. In short, net out-migration can result from an increase in gross out-migration combined with a decrease in gross in-migration. However, whatever the combination of these components, the net migration process both facilitates and is a response to expansion, which, in turn, adds to the emerging dominance of rings as the "hub" of metropolitan activity.

The lack of data for functionally demarcated areas makes it difficult to test directly propositions related to metropolitan development; however, our analysis of SMSAs tends to lend indirect support to the broadened theory of ecological expansion. Although our statistical analysis has been relatively simple (focusing primarily on two variable relationships) and greatly constrained by the boundary problems emphasized throughout, it has shown that two indicators of expansion (the percentages of SMSA jobs and of population in the ring) are associated with the extent and direction of net migration. Additional support for the theory was obtained through the confirmation of hypotheses relating specific types of jobs to migration and through the confirmation of hypotheses relating expansion to commuting flows and some commuting flows to net migration. In short, all of the hypotheses offered have been confirmed. In each case, however, our analysis has been limited not only by the far less than ideal indicators employed but also by the lack of fit of the operationalized component parts of the metropolitan area concept. These shortcomings, together with the generally moderate to low strength of the associations, indicate the conditions under which our results are tendered.

The results appear conclusive enough, however, to indicate the need for additional research. One beneficial project would be a similar study using more refined indicators of expansion and a longitudinal design. More re-

defined indicators of expansion would allow a clearer identification of the different roles played by the various dimensions of expansion in the process of population change. For instance, our analysis has suggested that the decentralization of coordinating and integrating functions lags behind that of other functions, but that the former play a particularly important role in initiating net population decline as a result of migration. It may well be that specific coordinating and integrating functions are more important than others, or that they operate only indirectly through factors which we have not considered, such as communication linkages or various patterns of residential segregation.

Equally important would be research directed at identifying the "crucial levels" of expansion and how these may vary when entire metropolitan areas are dominated by different sets of activities. For this purpose it may be useful to adopt a case study type of methodology selecting three or four different types of metropolitan communities and studying in greater detail the similarities and the differences among their patterns of expansion and patterns of population growth resulting from migration. It may be that certain unique patterns of core and ring population change are associated with various stages of expansion and that the pattern of net migration decline we have associated with metropolitan expansion is actually more a product and characteristic of one component part of the metropolitan community than of the whole area. That is, patterns of gross migration change in the core or the ring alone could account for the reversal of the traditional pattern in many areas.

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American Journal of Sociology

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The Deterrent Effect of Capital Punishment: New Evidence on an Old Controversy¹

David P. Phillips

University of California, San Diego

On the average, homicides decrease by 35.7% immediately following a publicized execution. The more publicity devoted to the execution, the the more homicides decrease thereafter. This decrease apparently occurs because capital punishment has a short-term deterrent effect on homicides. This deterrent effect has not been demonstrated previously. A long-term deterrent effect is not evident from the findings.

Capital punishment has just resumed in the United States, after a nearly unbroken moratorium of 12 years. More than 400 U.S. prisoners are currently awaiting execution. The appropriateness of capital punishment has been intensely debated for many years in many countries. Arguments on this topic have often been phrased in terms of whether capital punishment has a deterrent effect on homicide.² Until now no such deterrent effect has been found (Great Britain Royal Commission on Capital Punishment 1953; National Research Council 1978; Bowers 1974; Bedau 1967; U.N. Economic and Social Council 1973; Canada Department of the Solicitor General 1972; Wolfgang 1978). The U.S. National Academy of Sciences commissioned a panel to review the scientific literature on deterrence. Because of methodological flaws in this literature, the panel concluded that "the available studies provide no useful evidence on the deterrent effect of capital punishment" (National Research Council 1978, p. 9).

I now present what I believe to be the first compelling statistical evidence that capital punishment does deter homicides for a short time. The statistical techniques to be used in this paper are adapted from earlier analyses of short-term fluctuations in mortality (Phillips 1974, 1977, 1978, 1979; Phillips and Feldman 1973).

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² In this paper I will be concerned only with the deterrent effect of capital punishment. I will not consider other important dimensions of this topic, for example, the morality or immorality of capital punishment.

Recently only Ehrlich (1975*a*, 1975*b*) has claimed that capital punishment deters homicides. But Ehrlich's methodology has been widely criticized and his claims have not been generally accepted (Bowers and Pierce 1975; Passell 1975; Passell and Taylor 1975; Klein, Forst, and Filatov 1978). The failure to detect a deterrent effect of capital punishment is somewhat puzzling for two reasons: (1) psychological experiments show that people are often deterred from exhibiting aggression when they see someone else punished for it (Bandura 1973, 1977; Flanders 1968; Comstock 1975; Comstock and Fisher 1975; Liebert and Schwartzberg 1977);³ (2) there is anecdotal evidence that some criminals may have been deterred by the threat of capital punishment (Great Britain Royal Commission on Capital Punishment 1953).

Researchers may have failed to discover a deterrent effect because almost all studies on capital punishment use *yearly* rather than weekly or daily homicide data.⁴ If publicized executions deter homicides for a brief time only, the deterrent effect of capital punishment may not be detectable with yearly homicide data, only with weekly or daily data.⁵ In this paper I will use weekly homicide statistics for a 57-year period.

Sources of Data

A search of the vital statistics collections of the Library of Congress, the National Library of Medicine, and the British Museum uncovered only one published set of weekly homicide statistics in a country practicing capital punishment, England. This country published weekly homicide statistics for London, 1858–1921 (Great Britain General Register Office 1858–1921). Because of the paucity of the data available, the following study is restricted to one geographic area and one time period. One cannot necessarily generalize the findings of this study to other places and times.

A standard casebook of notorious murderers was used to generate a list of heavily publicized English executions, 1858–1921 (Wilson and Pitman

³ Almost all studies on the topic are based on experiments *in the laboratory*. Some researchers (e.g., Singer 1971) have noted that it may not be possible to generalize with confidence from behavior in the laboratory to behavior in the outside world.

⁴ Of course this is not the only methodological problem with research on the deterrent effect of capital punishment. Additional problems have been thoroughly discussed in the studies cited in the first paragraph of this paper and in Gibbs (1975) and Zimring and Hawkins (1973).

⁵ The few studies using daily or weekly data (Dann 1935; Graves 1956; Savitz 1968) are inconclusive. This may be because these studies examined very few cases and/or failed to restrict attention to highly publicized executions. For reviews of these three studies, see National Research Council (1978), Bowers (1974), and Bedau (1967). For an interesting analysis of Graves's work, see Gibbs (1975, pp. 192–94). Graves found possible evidence of a "brutalizing" effect of executions: on and just before the execution date there were *more* homicides than would be expected. However, he did not show that this result was statistically significant; hence it may be only a random fluctuation in the data. The statistics analyzed in the present paper provide no evidence of a "brutalizing" effect.

1962).⁶ Table 1 lists these executions (ranked by the amount of publicity each received) and the number of homicides before, during, and after the week of each execution.

Definitions to Be Used

The week in which the execution is publicized in the newspaper will be termed "the execution week" or "the experimental period."⁷ The week just *before* the experimental period and the week just *after* will be termed "the control period." Choosing a control period which straddles the experimental period allows one to correct for seasonal fluctuations and for linear trends over time. In addition, this method of choosing controls allows one to correct for fluctuations in homicides by day of the week, because the control period contains the same days of the week as does the experimental period.

The null hypothesis, H_0 , states that executions have no effect on the number of homicides. Given H_0 , there should be no tendency for the number of homicides in the experimental period to fall below the average weekly number of homicides in the control period. In contrast, the alternative hypothesis, H_{alt} , states that executions deter some homicides in the execution week. Given H_{alt} , the number of homicides in the experimental period should generally fall below the average weekly number of homicides in the control period.

RESULTS

Table 1, column 6, shows the average weekly number of homicides in each control period; column 7 indicates whether the number of homicides in the experimental period is less than the weekly average in the control period (indicated by a "-") or more than the average (indicated by a "+"). In three cases the number in the experimental period equaled the weekly average in the control period. In the remaining 19 cases, there are 15 "-" and four "+" signs. Given H_0 , the probability of 15 or more "-" and four or fewer "+" can be calculated from the binomial distribution ($P = .5$; $N = 19$; $X \geq 15$). This probability is .0096. Hence, there is a statistically significant tendency for the number of homicides to drop below the number expected in the week of a publicized execution.⁸

⁶ This volume was used in preference to alternative sources (Scott 1961; Hodge 1953; and the *Notable British Trials Series*) because it is far more comprehensive than the alternatives. I omitted publicized executions during World War I and the Boer War because war news overshadowed other types of news during these periods.

⁷ The choice of a one-week experimental period seemed plausible because earlier studies (Phillips 1977, 1978, 1979) suggested that some publicized events affect the level of mortality for approximately one week.

⁸ Following Phillips (1974, 1977, 1978, 1979) I have used nonparametric techniques throughout this paper, because the assumptions required by parametric tests are not met

TABLE 1
WEEKLY HOMICIDES BEFORE, DURING, AND AFTER PUBLICIZED
EXECUTIONS (London, 1858-1921)

NAME OF PERSON EXECUTED AND DATE OF EXECUTION STORY IN THE <i>Times</i> (1)	No. OF COL. INCHES DEVOTED TO THE STORY IN THE <i>Times</i> (2)	No. OF HOMICIDES IN WEEK OF EXECU- TION STORY (3)	CONTROL PERIOD		AVERAGE No. OF WEEKLY HOMICIDES IN CONTROL PERIOD (6)	DIFFERENCE BETWEEN OBSERVED No. OF DEATHS IN EXECUTION WEEK (Col. 3) AND No. EXPECTED (Col. 6) (7)
			No. of Homicides in Week before Experi- mental Period (4)	No. of Homicides in Week after Experi- mental Period (5)		
Wainwright (12/22/1875).....	1,694.5	0	1	0	.5	-.5
Crippen (11/24/1910).....	1,570.0	0	1	0	.5	-.5
Muller (11/15/1864).....	1,362.4	1	1	2	1.5	-.5
Webster (7/30/1879).....	972.9	0	2	5	3.5	-3.5
Mapleton (11/28/1881).....	828.8	2	6	2	4.0	-2.0
Lamson (4/29/1882).....	790.0	0	0	2	1.0	-1.0
Seddon (4/19/1912).....	614.3	1	2	1	1.5	-.5
Cream (11/16/1892).....	558.1	0	1	0	.5	-.5
Field and Gray (2/5/1921).....	553.8	0	0	0	.0	.0
Chapman (4/8/1903).....	389.5	0	1	2	1.5	-1.5
Peace (2/26/1879).....	363.9	0	1	2	1.5	-1.5
Read (12/5/1894).....	349.4	2	1	3	2.0	.0
Milsom and Fowler (6/10/1896).....	294.6	0	2	0	1.0	-1.0
Owen (8/9/1870)...	224.6	0	1	1	1.0	-1.0
Devereux (8/16/1905).....	215.1	1	3	0	1.5	-.5
Mackay (1/30/1913)	208.4	0	1	0	.5	-.5
Sheward (4/21/1869).....	199.6	6	2	1	1.5	+4.5
Dougal (7/15/1903).	154.3	1	2	0	1.0	.0
Brinkley (8/14/1907).....	129.6	2	1	0	.5	+1.5
Taylor (1/3/1883)..	111.3	2	0	0	.0	+2.0
Dickman (8/10/1910).....	101.1	0	1	0	.5	-.5
Stratton brothers (5/24/1905).....	91.0	1	0	0	.0	+1.0

The definition of the control period used above (the two weeks immediately surrounding the experimental period) is plausible but arbitrary. Other, equally plausible definitions could have been used, for example, (1) the four weeks immediately surrounding the experimental period, (2) the six weeks immediately surrounding the experimental period, or (3) the eight weeks immediately surrounding the experimental period. Whichever of these definitions is adopted, a marked drop in homicides is found in the execution week. When definition (1) is used and the binomial test applied to the data, the drop in executions is significant at .006; when definition (2) is used, the drop is significant at .039; when definition (3) is used, the drop is significant at .067.

Relationship between the Amount of Publicity Devoted to an Execution and the Decrease in Homicides Just after the Execution

The more publicity is devoted to an execution, the more likely it is that people will hear of it and be affected by it. Thus, if a publicized execution deters some homicides, the more publicity given to the execution, the more homicides should be deterred. Newspaper publicity devoted to an execution story can be measured systematically and exhaustively only for those newspapers whose contents are exhaustively indexed. The *London Times* is the only London newspaper indexed for the period 1858–1921. Because the *Times* had a smaller circulation than some other newspapers in London, it is not ideal for the purposes of this study. Nonetheless, it did devote much space to the executions under examination, and the number of column inches accorded by the *Times* to an execution story (table 1, col. 2) will be used as a rough indicator of the total amount of newspaper publicity devoted to each story.⁹

The Spearman rank correlation between publicity devoted to an execu-

by the data. Phillips (1970) discusses the rationale for applying significance tests, particularly the binomial, to the analysis of short-term fluctuations in mortality. For more general discussions of the significance test controversy, see Morrison and Henkel (1970). For examples of numerous applications of significance tests to time-series data, see any text on statistics for economists.

⁹ One might prefer to restrict attention to *front-page* coverage of the execution story, but this is not possible, because the *London Times* front page was devoted entirely to advertisements during the period under study. The *London Times* generally printed crime news in a particular section of the paper. Researchers have argued that a publicized execution should have its greatest effect if the murder, the trial, and the execution are all heavily publicized and occur within a short space of time. Consequently, the publicity devoted to an "execution story" has been defined as publicity devoted to the crime, the murderer, his trial, and his execution. Stories on these topics have been found through a systematic search of *Palmer's Index to the Times* (1864–1921), under the headings (1) murder, (2) police, (3) inquest, (4) name of victim, (5) name of murderer (including aliases), (6) nickname of the crime, (7) criminal trials, and (8) executions. The number of column inches devoted to these stories was measured to the nearest 1/8 of an inch on a "Superior" microfilm reader.

tion story (col. 2) and the size of the drop in homicides in the week of the execution (col. 6) is .546, corrected for ties ($P = .0035$) (see n. 8).

I have now shown that (1) homicides drop significantly in the week of a publicized execution, and (2) the more publicity given to the execution, the more homicides drop.¹⁰

Some Alternative Explanations of the Findings

These findings are consistent with the hypothesis that publicized executions trigger a brief decrease in homicides. As noted previously, the findings cannot be ascribed to the effect of weekday or seasonal fluctuations in the data or to linear trends, because these effects were corrected for in the selection and treatment of the control periods with which the experimental periods are compared. Furthermore, because the results are statistically significant, they cannot plausibly be ascribed to chance fluctuations in the data.

It might be claimed that the results in table 1 are caused by a regression effect of the following sort. Suppose that experimental-period mortality is relatively lower than control-period mortality, not because experimental-period mortality is unusually low, but because control-period mortality is unusually high. If this is so, experimental-period mortality may be lower than control-period mortality because of a regression toward the mean.¹¹

This "regression argument" is implausible for at least two reasons. First, it requires the assumption that control-period mortality is unusually high. This assumption is false. As we will see later (in fig. 1), average control-period mortality is if anything low, not high, in the two weeks immediately surrounding the experimental period. Second, the regression argument cannot be used to explain one aspect of the data in table 1: the more publicity given to the execution story, the more homicides drop in the experimental period. Thus, at present, the best available explanation of the findings is that publicized executions trigger a brief decline in homicides.

Although this explanation seems to be the best available, it is of course incomplete. One cannot confidently identify in detail the psychosocial processes linking the publicized execution with a drop in homicides. Several processes may conceivably be at work: (1) Homicides may decline during the execution week in part because potential murderers are deterred by fear. (2) In addition, homicides may decline because the publicized execu-

¹⁰ It would be interesting to determine whether publicized executions affect one type of homicide more than another; for example, premeditated murders might be more strongly affected than unpremeditated murders (e.g., crimes of passion). Similarly, one might conjecture that capital homicides would be more strongly affected than other types. These conjectures cannot be tested with the data under study because they are not subclassified by type of murder.

¹¹ For general discussions of problems arising from regression toward the mean, see Campbell and Stanley (1966) and Campbell (1974). For a specific discussion of regression toward the mean in homicide data, see Gibbs (1975).

tion reinforces community abhorrence of murder. Because of this increased abhorrence, a few potential murderers may be restrained, not by fear of punishment, but by moral considerations. (This type of process has been described generally by Durkheim [1964] and more specifically by Gibbs [1975], who termed it "normative validation.") The evidence in the next section throws additional light on the psychosocial processes that may be at work.

Weekly Fluctuation of Homicides before and after Executions

Figure 1 graphs the weekly fluctuation of homicides before and after the publicized executions in table 1. In figure 1, the average number of weekly homicides is 29.55; this can be taken as an estimate of the expected number of homicides in each week. In the week of the execution and in the week thereafter, the total number of homicides, $17 + 21 = 38$, falls 21.1 below the number expected, $2(29.55) = 59.1$. This corresponds to a drop in homicides of 35.7%, $100(59.1 - 38)/59.1 = 35.7\%$.

In the third, fourth, and fifth weeks following the execution week, the total number of homicides, $33 + 37 + 38 = 108$, rises 19.4 above the number expected, $3(29.55) = 88.65$. The drop in homicides immediately after the execution, 21.1, nearly matches the subsequent rise, 19.4.

The most plausible interpretation of this finding is that homicides are temporarily deterred for a two-week period; then the temporarily deterred homicides reappear after the publicized execution has faded from memory. Apparently the "lesson of the scaffold" is real, but only temporary.¹² This interpretation, if true, may have interesting scientific and public policy

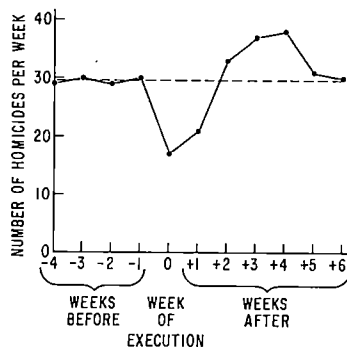


FIG. 1.—The frequency of weekly homicides before, during, and after 22 publicized executions, London, 1858-1921.

¹² One might conjecture that homicides decline after the fourth week following a publicized execution because a new publicized execution appears at this time. However, this conjecture is not supported by the evidence: there are no new publicized executions at this time or indeed in any of the six weeks following the execution stories in table 1.

implications. Some of these implications will be considered in the next section.

SCIENTIFIC AND PUBLIC POLICY IMPLICATIONS

Conservative versus Liberal Positions on Capital Punishment

It has generally been supposed that the conservative and liberal positions on capital punishment are irreconcilable. Conservatives maintain that capital punishment does have a deterrent effect; the reason that no such effect has been found to date is that insensitive research techniques have been used. In contrast, liberals maintain that capital punishment has no deterrent effect on homicides.

The data in this paper suggest that the conservative and liberal positions, apparently irreconcilable, are actually both correct. Conservatives are correct in that capital punishment has a short-term deterrent effect on homicides: figure 1 shows that in the two weeks following a publicized execution the frequency of homicides drops by 35.7%. On the other hand, in support of the liberal position, figure 1 also shows that capital punishment has no long-term deterrent effect on homicides. Within five or six weeks of a publicized execution, the drop in homicides is canceled by an equally large rise in homicides. In sum, conservatives are right in that there is a short-term deterrent effect, while liberals are right in that there is no net long-term effect.

Besides shedding light on the conservative-liberal controversy, figure 1 also helps to explain why previous studies on capital punishment uncovered no deterrent effect. As noted earlier, almost all of these studies used yearly homicide data. Figure 1 shows that the effect of a publicized execution is canceled within five or six weeks. In consequence, a publicized execution should generally have no net effect on the total number of homicides in the year containing the execution. Hence, a study using yearly homicide statistics should generally fail to uncover a brief deterrent effect of the type implied in figure 1. If this argument is correct, future studies of the deterrent effect of capital punishment should use daily or weekly data, not yearly statistics.

Potential Importance of Studies on Capital Punishment, and the Need for Replicative Studies

Executions have just resumed in the United States, and many branches of government are devoting time and attention to the controversies surrounding capital punishment. For example, legislators are proposing new laws or amending old ones, judges are considering appeals, and governors are deciding whether to commute death sentences.

British policy on capital punishment was shaped in part by American studies on the topic. It is conceivable, though hardly probable, that American policy will be influenced by the British data reported in this paper.¹³ It is far more likely that American policy will be influenced by American statistics on the effects of capital punishment. Consequently, it is important, even urgent, to determine whether the findings I have reported can be replicated with American data. These replicative studies are likely to be laborious and time-consuming, because the homicide statistics needed for such studies are not pretabulated as in the British case. It will be necessary to cull daily and weekly homicide statistics from coroners' offices and vital statistics bureaus.

Though laborious, studies with American data seem to be justified and are potentially very useful. These studies may shed some light on a scientific field which has been marked by 100 years of inconclusive results and a great deal of controversy. In addition, these studies have the potential to influence a key issue in American public policy—the debate over capital punishment.

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¹³ There are at least three reasons why one might not be able to generalize from London in 1858-1921 to contemporary America. In strong contrast to current U.S. experience, English justice seems to have been (1) swift, (2) certain, and (3) described in great, gruesome detail. Unlike contemporary American trials, the English trial usually took only three or four days, and the verdict was seldom appealed to another court. Consequently, English executions of the time generally occurred less than one month after the trial, and often the murderer was executed only a few months after his crime. During the trial English journalists provided detailed coverage, often including verbatim transcripts of parts of the proceedings. For much of the period under discussion, the execution was also described in great detail, sometimes including such repellent touches as "the prisoner hooked his feet over the steps of the scaffold and had to be dragged to the top."

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A Formal Theory of Selection for Schizophrenia¹

William W. Eaton

National Institute of Mental Health

A simple formal model of the selection of schizophrenics into social classes is developed, based on stochastic models of the process of occupational mobility. The model is estimated with data from England. Two predictions of the model are tested with data: the magnitude of the class differential in rate of schizophrenia at equilibrium, and the pattern of rates for classes in advantaged and disadvantaged ethnic groups. The model approximates the data in both tests. The major conclusion is that selection and drift together form a sufficient explanation for the class differential in rate of schizophrenia.

One of the most consistent findings in the field of psychiatric epidemiology is that lower-class persons have greater risk for schizophrenia. In a review of the 17 studies conducted throughout the world which relied on treatment facilities for case-identification, Eaton (1974) found that 15 observed the same inverse relationship between class and schizophrenia. The two exceptions, curiously, were studies conducted in rural areas. In a review of the seven studies conducted throughout the world which identified cases through community surveys, Dohrenwend (1975) found that five of the seven observed the inverse relationship. In a field as diverse and confused as psychiatric epidemiology, such consistency is impressive.

There are two very general interpretations for the relationship between class and schizophrenia. The first is called the "causation" interpretation because it holds that lower-class life somehow contributes to risk for schizophrenia, whether by stress (Dohrenwend and Dohrenwend 1974), stress and socialization together (Kohn 1977), or fetal damage (Pasamanick and Knobloch 1961). This interpretation is in line with the so-called environment argument concerning the ultimate causation of mental disorders. The second interpretation is called the "selection-drift" hypothesis. Drift is the process whereby schizophrenics undergo downward class mobility after the onset of schizophrenia due to disability in the competition for employment; it involves intragenerational mobility. Selection is the process whereby the insidious onset of schizophrenia handicaps an individual in the social class

¹ I would like to thank Professor Turner of the Department of Mathematics at McGill University for helping me with some of the problems involved in this work. H. B. Murphy, John Bartko, Gordon Allen, Larry Kessler, Rachel Rosenfeld, and François Nielsen provided invaluable comments on an earlier draft. Don Rae guided me through the necessary computer programs. None of these people are responsible for remaining errors.

system, so that his ultimate social class attainment is not as high as that of persons from the general population with similar background characteristics; it involves intergenerational mobility. Both the selection and drift theories are in line with the so-called heredity perspective concerning the ultimate causation of mental disorders.

So far the evidence supports the selection-drift interpretation. The causation arguments make sense theoretically, but there have been no data to support them directly, even though several studies have been designed to test them. There is fairly conclusive evidence in favor of the selection-drift interpretation. In a review of the 11 studies of social mobility and schizophrenia, Eaton and Lasry (1978) found that seven of them observed either downward mobility or lower-than-expected upward mobility among schizophrenics. None of the four remaining studies flatly contradicted the result with a finding of increased upward mobility among schizophrenics; they merely failed to observe the downward mobility. Furthermore, the mobility finding was strongest in the more careful and methodologically exacting studies. These results do not exclude the additional possibility of class causation, but they do show that schizophrenics are disabled in the modern occupational system.

The evidence on selection and drift processes is consistent with the increased awareness of the important role of genetics in schizophrenia. Very few researchers now deny the importance of genes, and some estimate the heritability to be quite high (Rosenthal 1971).

A QUANTITATIVE MODEL OF THE SELECTION-DRIFT PROCESS

The purpose of this paper is to outline a simple quantitative model of the process of selection and drift for schizophrenia, to make straightforward predictions from the model, and to discuss the implications of these predictions for future research in schizophrenia and stratification. The model is an application of techniques used in sociology to study the mobility of individuals in the general population (Bartholomew 1971).

The model begins with difference equations which express the flow of individuals from their social class of orientation (parental) to their social class of destination. If we arbitrarily divide the class structure into three classes, each class is made up of individuals originating from its own and the other two classes:

$$P_{1, n+1} = a_{11}P_{1,n} + a_{12}P_{2,n} + a_{13}P_{3,n};$$

that is, the proportion P in class 1 in generation $n + 1$ will equal the proportion a_{11} of those in class 1 in the n th generation who stay in class 1 plus the proportion a_{12} of those in class 2 in the n th generation who move to class

1 in the next generation plus the proportion a_{13} who move from class 3 to class 1 in the same generational time. Class 2 in the $n + 1$ generation is likewise made up of recruits from itself and the other two classes, and class 3 is made up similarly. For this simple structure with three classes, these three difference equations can be replaced by a simple three-entry row vector representing the class structure at any given time, and a 3×3 transition matrix: $\underline{P}_{n+1} = \underline{P}_n \underline{A}$.

This matrix equation has several interesting properties which we will discuss later. For now, let us estimate the \underline{A} and \underline{P} matrices for schizophrenics. The classic study of the selection process is by Goldberg and Morrison (1963). They provide a 5×5 father-to-son occupational mobility matrix for a population of schizophrenics. Collapsing their classes 1 and 2 into one upper class (1) and their classes 4 and 5 into one lower class (3) increases the sample size in each cell and fits this simple model. The result is the estimation of the entire father-to-son process shown below.

$$\begin{array}{ccc} \underline{P}_n & & \underline{A}_s & & \underline{P}_{n+1} \\ [.14 & .52 & .33] & \begin{bmatrix} .37 & .47 & .16 \\ .07 & .57 & .36 \\ .02 & .42 & .56 \end{bmatrix} & = [.10 & .50 & .40] \end{array}$$

Thus, for instance, the parental generation \underline{P}_n was made up of 14% in the upper class, 52% in the middle class, and 33% in the lower class. The class structure of the generation of offspring \underline{P}_{n+1} has shifted downward, so that, for example, 40% of this generation are in the lower class. The shift is explained by the transition matrix; for instance, the lower class takes 16% from the upper class, 36% from the middle class, and 56% from itself. The diagonal entries of the \underline{A} matrix might be referred to as the stability coefficients, because they are the proportions remaining in the class of origin. Below the diagonal the coefficients represent upward mobility, which is very minimal into the upper class (.07 from the middle class and .02 from the lower class) but sizable into the middle class from the lower class (.42). Above the diagonal downward mobility is represented in similar fashion, and the coefficients are larger. The transition matrix is labeled \underline{A}_s to stand for the selection process, since it portrays occupational mobility from father's occupation to the occupation of the son at the time of his first admission to treatment for schizophrenia.

This model is not complete because it does not include the intragenerational drift process. The Goldberg-Morrison data are not adequate for this purpose, but a study by Cooper (1961) is available. Cooper's research was conducted in England in the 1960s and used the same occupational categories as did Goldberg and Morrison; therefore the two studies are relatively comparable. For five to 10 years, Cooper followed first admissions diagnosed

as schizophrenic; he recorded their occupations at follow-up. In each class there were individuals who were unemployed at the time of follow-up and could not be classified easily. It is probably correct that many of those unemployed at follow-up were chronically unemployed and should be registered in class 3. However, a significant proportion of schizophrenics suffer recurrent episodes with periods of normalcy between, and a considerable proportion appear to remain in their class of origin. Therefore, at the time of follow-up half of the unemployed have been placed in their class at first admission and half in class 3 (for a review of data see Turner [1977]). The resulting transition matrix for drift, labeled \underline{A}_d , is shown below.

$$\underline{A}_d = \begin{bmatrix} .85 & .05 & .10 \\ .01 & .69 & .30 \\ .00 & .03 & .97 \end{bmatrix}$$

Comparing the two transition matrices, we can see that the drift process is stronger than the selection process in the case of these data; for instance, there is virtually no upward mobility after the onset of schizophrenia, although there is considerable stability in occupation.

The entire mobility process can now be represented as $\underline{P}_{n+1} = \underline{P}_n \underline{A}_s \underline{A}_d$. Taking the product of these matrices amounts to following the schizophrenic from his birth to his first admission, then to follow-up, and finally into the next generation. The resulting transition matrix \underline{A} is shown below.

$$\underline{A} = \begin{bmatrix} .32 & .47 & .21 \\ .06 & .52 & .42 \\ .03 & .42 & .55 \end{bmatrix}$$

The single matrix is more informative and will be more useful in discussing implications of the model below. Nevertheless, it is handy to recall that its components represent different processes; for instance, if an effective new treatment were introduced, it would influence the drift matrix \underline{A}_d but probably not the selection matrix \underline{A}_s . An effective screening device might be able to change the selection matrix but probably would not have much effect on the drift matrix.

At this point a comparison with the mobility of the general population will be helpful in understanding these processes. The best source of data is the study by Glass and Hall presented in Bartholomew, with the data from England in the 1950s presented below.

$$\begin{bmatrix} .60 & .28 & .12 \\ .23 & .47 & .30 \\ .13 & .38 & .49 \end{bmatrix}$$

The normal and schizophrenic matrices can now be combined. They derive from the same country in roughly the same period and employ iden-

tical categories of occupation. A generational transition matrix which ignores the class structure is shown next.

		Son	
		N	S
Father	N	.99	.01
	S	.90	.10

These estimates are based on two sources. The top row comes from the epidemiologic surveys that estimate the lifetime risk of schizophrenia to be about 1% (Babigian 1975). The second row comes from genetic studies of offspring of schizophrenics, which estimate the risk in this group at about 10% (Rosenthal 1971). Incorporating the class structure yields the matrix \underline{A}_{sn} shown below.

\underline{A}_{sn}						
	.59	.28	.12	.0032	.0047	.0021
	.23	.46	.30	.0006	.0052	.0042
	.13	.38	.48	.0003	.0042	.0055
	.54	.25	.11	.032	.047	.021
	.21	.42	.27	.006	.052	.042
	.12	.34	.44	.003	.042	.055

The matrix \underline{A} is divided into quadrants which let us discuss the sources of estimates. The upper left quadrant is from the father-to-son matrix of the general population of England, discussed above. Figures in each row have been lowered proportionate to their size so that the rows in this quadrant add to .99, with .01 transferring to the schizophrenic population. The upper right quadrant is the selection-drift matrix estimated above, with the decimal set to represent the overall rate of schizophrenia in the general population (.01). The lower left quadrant represents the class mobility of sons of schizophrenics and is taken from the normal mobility matrix (the upper left quadrant) with proportionate subtraction from each figure in each row so that the rows in this quadrant add to .90, to allow for transition into the schizophrenic population. The lower right population represents the mobility of schizophrenics and is thus similar to the selection-drift matrix, except that the decimal has been moved to reflect the higher (by a factor of 10) risk of schizophrenia in this population of offspring of schizophrenics.

IMPLICATIONS OF THE MODEL

The Equilibrium Distribution

If the mobility processes operate in the same manner for a long period of time, the class structure of the population will eventually stabilize with a given proportion of the population in each of the three classes, and it can be proved that under certain conditions the transition matrix will totally deter-

mine the class structure, given enough time, no matter what the structure looks like in the beginning (Grossman and Turner 1973). As n approaches infinity, there is a limiting value of the n th power of the matrix \underline{A} and an associated structure vector \underline{P} , which is sometimes called the equilibrium vector. One of the reasons for choosing the English data of Glass and Hall, and the data on schizophrenics from England in the same period, was that the equilibrium value of \underline{P} was very close to the observed class structure—that is, the English class structure was roughly at equilibrium during this period (Bartholomew 1971, pp. 33–35).

The matrix \underline{A} has an equilibrium solution,

$$\underline{P}_n = [.31 \quad .38 \quad .30 \quad .0014 \quad .0052 \quad .0044],$$

which is the predicted proportion of normal persons and schizophrenics in the three classes if the population is at equilibrium. From these proportions we can calculate the equilibrium rates of lifetime prevalence of schizophrenia specific to the three classes, as in table 1, column 1. (For example, the upper-class rate of .0045 results from dividing .0014 by .31.) These rates are at a level that is consistent with what is known about schizophrenia—that is, about 1% lifetime prevalence—but the 1% figure was incorporated into the model in the beginning so there is no real test of fit in that correspondence. However, the class difference in rates was not part of the original model and serves as a check on its adequacy. Fortunately independent data are available from the Registrar General of England on the class distribution of schizophrenia for about the same period (1949–53) and using the same occupational categories (table 1, col. 2). The level of the rates is much lower because they represent first admissions over a four-year period, and not lifetime prevalence as in the equilibrium rates. The class differential is similar in magnitude: the rate in the lower class is about 2.6 times that in the upper class. Some, but not all, other studies give roughly the same 3:1 ratio of lower- to upper-class rates (e.g., Hollingshead and Redlich 1958).

Disadvantaged Ethnic Groups

Dohrenwend and Dohrenwend have argued that the differential opportunity structures for different ethnic groups provide a natural experiment on the environment-heredity issue, or, more accurately, what they call the “stress-selection” issue (1969). The selection prediction is what concerns us here. Even if the overall rates of schizophrenia in the black and white populations are equivalent, the rates in the different social classes need not be equal for blacks and whites because both being schizophrenic and being black hinder upward mobility. The upwardly mobile whites tend to leave a residue population which has a high rate of schizophrenia, whereas healthy and able blacks are held in lower-class position, diluting the rate of schizophrenia so that it is lower than the (class-constant) white rate.

TABLE 1
CLASS-SPECIFIC RATES OF SCHIZOPHRENIA

CLASS	ENGLAND	
	Predicted Lifetime Prevalence at Equilibrium	Observed Rates of First Admission (Four Years)
1 (upper).....	.0045	.0006
2 (middle).....	.014	.0009
3 (lower).....	.015	.0016

There were few or no large, easily identifiable ethnic groups in Britain in the 1950s for which mobility data are available and which were disadvantaged as were blacks in the United States. There are transition matrices on blacks and whites in the United States, showing that blacks have much lower upward mobility and greater downward mobility than whites (Duncan, Featherman, and Duncan 1971). Also, the data for whites yield a pattern similar to that for Britain. It is, therefore, possible to create a matrix for an ethnic group in England that suffers disadvantages similar to those of blacks in the United States. In order to do this, the way in which the two statuses of "disadvantaged ethnic group" and "schizophrenic" combine must be hypothesized. Being schizophrenic is perhaps so disabling that belonging to a disadvantaged ethnic group is not a further hindrance to mobility, in which case only the mobility of normal persons from the disadvantaged ethnic group is changed in A_{sn} . On the other hand, the statuses may be additive, so that schizophrenics from disadvantaged ethnic groups are more disabled than their majority-group schizophrenic counterparts, in which case the mobility of schizophrenics and normal persons in the A_{sn} matrix would be lowered proportionately. The first case will be labeled the master-status model, since it assumes that the status of schizophrenic overwhelms other statuses; the second case will be called the additive model.

The equilibrium solutions are shown below.

Master-status model:

$$P_n = [.08 \quad .18 \quad .73 \quad .0008 \quad .005 \quad .005].$$

Additive model:

$$P_n = [.06 \quad .21 \quad .72 \quad .00012 \quad .0020 \quad .0080].$$

The two models are nearly identical in the normal populations but quite different in the schizophrenic populations. The rates resulting from each model (table 2) are very different; in the master-status model, the relationship of class to schizophrenia is not even linear: the highest rate is in the middle class, not the lower class. For the lower class both models predict that the advantaged group will have a higher rate of schizophrenia than the

disadvantaged group, as in the Dohrenwend and Dohrenwend selection prediction. But the master-status model predicts that comparison only at the lowest level of class, whereas the additive model predicts it at all levels of class. In the master-status model there is a peculiar flip-flop at the middle and upper class, where the disadvantaged group has higher rates. The flip-flop is due to the comparatively high upward mobility of normal persons in the advantaged group and the comparatively lessened downward mobility of schizophrenics in the disadvantaged group.

The only available data comparing rates of schizophrenia among blacks and whites with social class held constant are those published by Jaco (1960), which are reproduced in columns 1 and 2 of table 3. The overall rate of schizophrenia for whites is on the same order of magnitude as that in the English data—a little less than one per 1,000. The class differential is clear for whites in Texas but not as strong as in England, while the class differential for blacks in Texas is curvilinear. In the Texas data, in the lowest class the advantaged group has a higher rate of schizophrenia than the disadvantaged group, indicating the more rapid formation of residues in the advantaged group. The picture is reversed in the upper class, in which the dis-

TABLE 2
PREDICTED CLASS-SPECIFIC RATES OF SCHIZOPHRENIA
IN ADVANTAGED AND DISADVANTAGED
ETHNIC GROUPS

CLASS	ADVANTAGED GROUP	DISADVANTAGED GROUP	
		Master-Status Model	Additive Model
1 (upper).....	.0045	.0092	.0020
2 (middle).....	.014	.028	.0095
3 (lower).....	.015	.0068	.011

TABLE 3
CLASS-SPECIFIC RATES OF SCHIZOPHRENIA
OBSERVED IN TEXAS
ANNUAL FIRST ADMISSIONS (Males)

	TEXAS	
	White	Nonwhite
Class 1.....	.00039	.00058
Class 2.....	.00051	.00092
Class 3.....	.00074	.00052

SOURCE.—E. G. Jaco, *The Social Epidemiology of Mental Disorders* (New York: Russell Sage, 1960), table 15.

NOTE.—Class 1 consists of the average rate of professionals and managers; class 2 of the average rate of clerical, sales, and service workers; class 3 of the average rate of agricultural and manual workers and unemployed.

advantaged group has the higher rate. Thus, the data from Texas suggest the flip-flop pattern of the master-status model.

CONCLUSION

This model does not suggest that environmental influences have no effect on schizophrenia. It only suggests that the class differential can be completely explained by a selection-drift model and that if some kind of stress affects the development of schizophrenia, it is a sort of stress which is *not* necessarily related to social class. This suggestion is supported by several studies which have tried to link various indicators of stress to standard measures of social class, such as education, occupation, and income. Many of these studies (e.g., Eaton 1976; Dohrenwend 1973) have come up with surprisingly small relationships; it seems that even some standard measures of stress are not systematically related to social class. Certainly there are many types of stress and emotional upset that have been related to schizophrenia but are not necessarily more frequent in the lower class, such as the emotionally involved family mentioned by Brown, Birley, and Wing (1972) or the schizophrenia-evoking role of complex social tasks developed by Murphy (1972). The direction of research suggested here is to look for *class-constant* stressors, not stressors that are more frequent in the lower class, such as events related to the economy.

These models may be applicable to other statuses or diseases related to social class. The important point illustrated here is that in comparing class-constant rates two separate mobility processes are involved—one from the population yielding the rate numerator, and the other from the population yielding the denominator. Either or both mobility processes can affect the observed rates.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

THE "COMMON STRUCTURE SEMIGROUP," A REPLACEMENT FOR THE BOORMAN AND WHITE "JOINT REDUCTION"¹

The work of Harrison C. White and his co-workers Scott A. Boorman and Ronald L. Breiger presented in the two-part article "Social Structure from Multiple Networks" (*AJS* 81 [January 1976]: 730-80; *AJS* 81 [May 1976]: 1384-1446) has aroused a lot of interest, especially among those interested in networks. However, only half of the technique has been used, that presented by White, Boorman, and Breiger in part 1, "Blockmodels of Roles and Positions." A number of studies have used the algorithm CONCOR to block people into sets with similar relations to others (Breiger 1976; Mullins et al. 1977). However, this is not the most distinctive, interesting, and ambitious part of the blockmodel approach. The second half of the technique, unfortunately used hardly at all, involves describing structural relations with algebraic semigroups. This comment is addressed to problems in the second half of the technique as developed by Boorman and White in part 2, "Role Structures."

The "joint reduction" is a very important part of the use of algebraic semigroups to characterize group structure. The joint reduction is a way Boorman and White suggest for abstracting the underlying common findings in two or more studies. It is clearly the culmination of their technique. This

¹ I would like to acknowledge the helpfulness of the comments and suggestions of K. Jill Kiecolt, John Light, and Maureen J. McConaghy.

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comment will show first that, contrary to Boorman and White's claim, the joint reduction of the findings from two or more studies does not always exist and, second, that the joint reduction as defined by Boorman and White does not do what it is supposed to do: it does not abstract the common aspects of two or more studies. I propose instead a "common structure semigroup" that always exists and that does give the common structural aspects of two or more studies.

First, a very brief review of what Boorman and White propose. All the possible Boolean matrix products of a set of generating binary relational matrices form an algebraic "semigroup"; a semigroup is any set of elements (the relational matrices and their products) and a binary associative operation (Boolean matrix multiplication). The findings of a study using blockmodeling are simply the equations in the semigroup generated by a set of relational matrices. For example, if P and N were matrices of positive and negative choices, then the equation $P^2 = P$ would imply that the positive relation is transitive ("friends of friends are friends"). The equation $N^2 = P$ would mean that enemies of enemies are friends, a finding consistent with balance theory (Taylor 1970). The semigroup generated by a set of relational matrices may be too complex to analyze readily. Homomorphisms simplify a semigroup (or any other algebra). A homomorphic image is formed by equating elements of a semigroup. The equations imposed on a semigroup in order to simplify it can be suggested by the data (p. 1408); two matrices might be equated because they are quite similar. Or the simplifying equations might be those that appear in a variety of studies that include the same generating relations. This simplification is the "joint reduction."

1. *The joint reduction does not always exist because generator-preserving homomorphisms do not form a lattice.*

First, let us review what a joint reduction is and what it is supposed to accomplish. The joint reduction of two different semigroups is meant to be an algebraically simpler version of what two semigroups with the same generators have in common. The joint reduction is simpler in that it always contains as few distinct elements as either of the semigroups from which it is derived, or fewer than either.

The joint reduction is based on the concept of a generator-preserving homomorphism (pp. 1419–21). A generator-preserving homomorphism from one semigroup, S_1 , to another, S_2 , is a homomorphism which is an "identity mapping on generators" (p. 1419). There is a one-to-one correspondence between the generators of the two semigroups, S_1 and S_2 . If h is the mapping from S_1 to S_2 and a is a generator of S_1 , then $h(a) = a$. What is ruled out is a combining of generators; two generators of S_1 cannot be mapped into the same element of S_2 by a generator-preserving homomorphism. If S_2 is a

generator-preserving homomorphic image of S_1 , this fact is written as $S_1 \subseteq S_2$. The relationship \subseteq is a partial order.

It is claimed (p. 1420) that \subseteq implies a lattice structure among semigroups with the same generators (a lattice exists if every pair of elements has a least upper bound and a greatest lower bound). Consider the set of semigroups that are generator-preserving homomorphic images, or "reductions," of both S_1 and S_2 . The claim is made that this set has a greatest lower bound; that is, there is a semigroup S_{GLB} that is a generator-preserving homomorphic image of both S_1 and S_2 ($S_{GLB} \subseteq S_1$ and $S_{GLB} \subseteq S_2$) and that S_{GLB} ranks above any other homomorphic image of both S_1 and S_2 ($S_3 \subseteq S_1$ and $S_3 \subseteq S_2$, then $S_3 \subseteq S_{GLB}$). The joint reduction of S_1 and S_2 is S_{GLB} . Since homomorphisms are thought of as "cruder" or "coarser" versions of an algebra, S_{GLB} is the finest semigroup that is a generator-preserving simplification of both S_1 and S_2 .

However, as defined, the joint reduction of two semigroups may not exist because generator-preserving homomorphisms and the relation \subseteq do not in fact have a lattice structure. To see this, let us review lattices of congruence relations. A semigroup S with generator set G can be thought of as a partition P of the free semigroup with generators G , $FS(G)$. Two elements of $FS(G)$ are equivalent if they are both equal to the same element in S . Every semigroup with generators G corresponds to a partition of $FS(G)$, although not every partition of $FS(G)$ corresponds to a semigroup. Those equivalence relations (partitions) that correspond to semigroups are called congruence relations. All the partitions of $FS(G)$ form a partition lattice in which $P_1 \geq P_2$ means that P_1 is a "coarser" version of P_2 (all its equivalence classes are either the equivalence classes of P_2 or unions of the equivalence classes of P_2). With respect to the relation \leq , $P_1 + P_2$ (the "join" of P_1 and P_2) is the least upper bound of P_1 and P_2 , and $P_1 P_2$ (the "meet" of P_1 and P_2) is the greatest lower bound.

In the partition lattice of $FS(G)$, the congruence relations form a sublattice (Birkhoff 1967, p. 138). This means that if P_1 and P_2 are congruence relations corresponding to semigroups S_1 and S_2 , then $P_1 + P_2$ and $P_1 P_2$ are also congruence relations that correspond to semigroups generated by G . Since there is a one-to-one correspondence between congruence relations and semigroups, let us write $S_1 \leq S_2$ if $P_1 \leq P_2$, and let $S_1 + S_2$ and $S_1 S_2$ be the semigroups corresponding to $P_1 + P_2$ and $P_1 P_2$, respectively. It can be shown that $S_1 \leq S_2$ if and only if S_2 is a homomorphic image of S_1 . Thus, $S_1 + S_2$ is the least upper bound of all semigroups that are homomorphic images of both S_1 and S_2 .

In the Boorman and White paper $S_1 \subseteq S_2$ means that S_1 is a generator-preserving homomorphic reduction of S_2 , but since S_1 is a homomorphic image of S_2 , $P_1 \geq P_2$ in the congruence lattice of $FS(G)$ and $S_1 \geq S_2$. It is

confusing that Boorman and White reverse the conventional lattice partial order in defining their relation \leq . The joint reduction of S_1 and S_2 , called S_{GLB} in the paper, would more conventionally be called S_{LUB} , but we will stick to their symbol S_{GLB} for the joint reduction.

Since S_{GLB} is a homomorphic reduction of S_1 and S_2 and $S_1 + S_2$ is the least upper bound of S_1 and S_2 , it follows that $S_{GLB} \geq S_1 + S_2$. In the sublattice of congruence relations of $FS(G)$, S_{GLB} , being higher than $S_1 + S_2$, corresponds to a "coarser" partition of $FS(G)$; S_{GLB} may equate elements of $FS(G)$ that are unequal in the partition corresponding to $S_1 + S_2$.

Now, suppose that $S_1 + S_2$ is not a generator-preserving homomorphism of S_1 and S_2 , so that two or more generators in S_1 and S_2 are mapped into the same elements in $S_1 + S_2$. Since $S_{GLB} \geq S_1 + S_2$, it follows that S_{GLB} must also combine these generators. But this is a contradiction because S_{GLB} is, by definition, a generator-preserving homomorphic image of S_1 and S_2 . Therefore, whenever $S_1 + S_2$ is not generator-preserving the joint reduction of S_1 and S_2 does not exist.

The last step is to show that there are instances in which $S_1 + S_2$ is not generator-preserving. As an illustration, consider semigroup III (p. 1407), reproduced below.

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>
<u>1</u>	1	2	3	4
<u>2</u>	3	4	3	4
<u>3</u>	3	4	3	4
<u>4</u>	3	4	3	4

This semigroup has two generators, 1 and 2, so the four elements can be identified with four equivalence classes of $FS(1,2)$. Consider all congruence relations of $FS(1,2)$ that are coarser than this partition. These are the congruence relations of all homomorphic images of semigroup III. They are 1/2/3/4, 24/1/3, 34/1/2, 13/24, 1/234, and 1234. Let the corresponding semigroups be S_1 (which is semigroup III), S_2 , S_3 , S_4 , S_5 , and S_6 , respectively. The sublattice of these congruence relations is shown in figure 1.

Now the crucial point. What is the joint reduction of S_3 and S_4 , or of S_4 and S_5 ? It does not exist because $S_3 + S_4 = S_4 + S_5 = S_6$, and S_6 does not maintain the distinction between the two generating elements 1 and 2. Generator-preserving homomorphisms do not imply a lattice structure. The mathematical reference cited in the Boorman and White paper (Birkhoff 1967) shows only that congruence relations form a sublattice of the partitions of $FS(G)$, so that all homomorphisms give a lattice structure, but generator-preserving homomorphisms need not.

2. Joint reductions are not the common truth in two semigroups.

The joint reduction of two semigroups is supposed to be what they have "in common." A homomorphic image is a simpler, less detailed version of a semigroup. The joint reduction of two semigroups is thus the most detailed

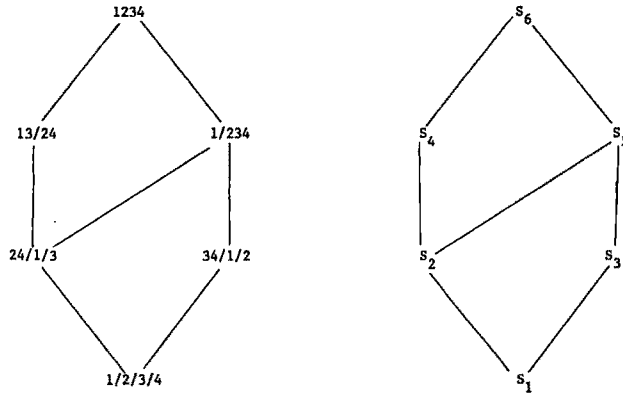


FIG. 1.—Congruence lattice for semigroup III

semigroup that is a simplified picture of both semigroups. In this sense, it is what they have in common. This is the way Boorman and White must have reasoned, but there is a serious mistake in this strategy, as I shall try to make clear.

A semigroup can be defined by its multiplication table or by a set of equations that operate as restrictions on the free semigroup $FS(G)$. These equations are the “findings” or “laws” that the semigroup describes.

For example, consider semigroups II, III, and IV on page 1407. Let the two generators be a and b . The five elements in semigroup II can be called a , b , ba , ab , and b^2 —as Boorman and White point out, such labeling is arbitrary since each row and column could just as well be labeled with other elements in the same equivalence class of $FS(G)$ —and the four elements of III can conveniently be labeled a , b , ba , and b^2 .

One set of equations producing semigroup II is: $a^2 = a$; $aba = b^2$; $bab = b^2$.

A set of equations producing semigroup III is: $a^2 = a$; $ab = b$; $b^2a = ba$.

A set of equations producing semigroup IV is: $a^2 = a$; $ab = b$; $ba = b^2$; $b^3 = b^2$.

The joint reduction of semigroups II and III, semigroup IV, corresponds to the *join* of the partitions of the free semigroup $FS(a, b)$ which correspond to semigroups II and III. Therefore, every equation true in II or in III is true in their joint reduction IV. But IV will contain equations that are true in *only one* of the two semigroups because it is defined by the union of the equations in II and III. In other words, the joint reduction, far from stripping away the idiosyncratic truths and relations in each semigroup in order to discover the common truths, contains all the findings, reliable and unreliable, from both studies.

Worse yet, the joint reduction will contain truths specifically contradicted

by both studies. These are truths (equations) supported by no data but requiring equations from both semigroups in order to be deduced. For example, the equation $ba = b^2$ is true in the joint reduction of II and III, but it is not true in either II or III.

This is clearly a bad way to find the common threads in diverse studies. Why was this choice made? Probably because the focus was on multiplication tables instead of the equations producing them. The multiplication table for a joint reduction will always have as few elements as either starting semigroup, or fewer. It will be simpler in that it will contain fewer elements, but the smallness of the joint reduction is achieved only at the expense of a large number of assumptions. The number of elements in a multiplication table and the number of "laws" it assumes will tend to be inversely related. A "small" multiplication table is actually very rich in assumptions and not simple but complex.

There is a solution. The common findings in a set of semigroups with the same generators are given by the semigroup which is their "product" rather than their "sum." More specifically, if S_1 and S_2 are semigroups and P_1 and P_2 are their associated congruence relations on the free semigroup, then P_1P_2 is also a congruence relation, and we can refer to the corresponding semigroup as S_1S_2 . The latter, which will be called the "common structure semigroup," contains only the equations that are true in both S_1 and S_2 , nothing more. In other words, S_1S_2 does exactly what the "joint reduction" is supposed to do: it identifies what is common in S_1 and S_2 because it is the intersection of the equations implied by S_1 and S_2 .

Referring again to semigroups II and III, although in the two lists of equations there is only one equation ($a^2 = a$) sufficient to produce the two semigroups, the common structure semigroup $S_{II}S_{III}$ shows that they have much more than this in common. When the approach described in the Appendix is used, $S_{II}S_{III}$ has the form shown below.

	<u>a</u>	<u>b</u>	<u>ab</u>	<u>ba</u>	<u>b²</u>	<u>aba</u>
<u>a</u>	a	ab	ab	aba	b ²	aba
<u>b</u>	ba	b ²	b ²	aba	b ²	aba
<u>ab</u>	aba	b ²	b ²	aba	b ²	aba
<u>ba</u>	ba	b ²	b ²	aba	b ²	aba
<u>b²</u>	aba	b ²	b ²	aba	b ²	aba
<u>aba</u>	aba	b ²	b ²	aba	b ²	aba

One set of equations producing this table is: $a^2 = a$; $b^3 = b^2$; $bab = b^2$; $b^2a = aba$.

These equations and their implications are consistent with both semigroups II and III. In both semigroups friends of friends are friends. In both semigroups negativity is characterized by the weak transitivity of the second equation. The third equation can be interpreted to mean that one's

enemies and their friends share similar enemies. The fourth is harder to interpret. As noted before, $S_{II} + S_{III}$, semigroup IV in figure 8 (p. 1407), contains principles supported by only one semigroup, for example, that enemies of friends are enemies ($ab = b$), or by neither semigroup ($ba = b^2$).

Summary

Some procedure for finding the common principles in a set of semigroups is essential in blockmodeling. Different studies will tend to produce different semigroups, and what they have in common will not always be obvious. There must be some way of abstracting out the common findings. However, the technique suggested by Boorman and White exhibits two problems. First, their joint reduction does not always exist. Second, it does not do what it purports to do. What Boorman and White want of the joint reduction is clear (p. 1406): "In substantive terms, the joint reduction is the outcome of abstracting structure common to both original role systems; it is the common denominator of the two role structures being compared."

Continuing the metaphor, the joint reduction that Boorman and White propose is actually closer to the least common multiple than to the greatest common divisor because it is the union of all the principles contained in the two subgroups rather than their common intersection. Moreover, the joint reduction will contain implications contradicted by both semigroups.

This comment proposes a "common structure semigroup" that has neither of these problems. The common structure semigroup of two or more semigroups is the semigroup corresponding to their meet in the lattice of congruence relations. This semigroup has only those equations that are present in all the initial semigroups. Moreover, this semigroup always exists, which is not true for the joint reduction.

APPENDIX

How to Construct the Common Structure Semigroup

Let S_a and S_b , consisting of the elements $\{a_1, a_2, \dots, a_r\}$ and $\{b_1, b_2, \dots, b_r\}$, be two finite semigroups with the same generating set $G = \{g_1, g_2, \dots, g_k\}$. Form all ordered pairs (g_i, g_j) . These ordered pairs are the generators of $S_a S_b$, where multiplication is defined as: $(a_i, b_j)(a_k, b_l) = (a_i a_k, b_j b_l)$.

As an illustration, consider the two semigroups with generators p and n shown below.

S_a			S_b		
	\underline{p}	\underline{n}		\underline{p}	\underline{n}
\underline{p}	p	n	\underline{p}	p	n
\underline{n}	n	p	\underline{n}	n	n

The two pairs (p, p) and (n, n) generate the three-element semigroup presented below.

	(p, p)	(n, n)	(p, n)
(p, p)	(p, p)	(n, n)	(p, n)
(n, n)	(n, n)	(p, n)	(n, n)
(p, n)	(p, n)	(n, n)	(p, n)

In $S_a S_b$, (p, p) and (n, n) can be identified with p and n , respectively, and $(p, n) = (n, n)(n, n)$ can be identified with n^2 . Therefore, $S_a S_b$ has the form shown below.

	<u>p</u>	<u>n</u>	<u>n^2</u>
<u>p</u>	p	n	n^2
<u>n</u>	n	n^2	n
<u>n^2</u>	n^2	n	n^2

PHILLIP BONACICH

University of California, Los Angeles

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ALGEBRAIC APPROACHES TO THE COMPARISON OF CONCRETE SOCIAL STRUCTURES REPRESENTED AS NETWORKS: REPLY TO BONACICH¹

A fundamental problem in structural sociology is to develop systematic and objective methods for comparing concrete social structures encountered in different populations. One important application of blockmodels is to suggest a rigorous approach to this problem, which the literature has previously addressed in ad hoc ways (see, e.g., Graham 1978). The paper by Boorman and White (1976; hereafter abbreviated B&W), on which Bonacich comments above, develops substantive and technical aspects of such a blockmodels-based approach. The first step is to construct blockmodels from multiple social network data in the populations being compared

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(see Arabie, Boorman, and Levitt [1978] for a review of procedures). The second step is the generation of a *blockmodel algebra* for each blockmodel so constructed (formally, this algebra is the Boolean matrix semigroup generated by the [binary] image matrices of the blockmodel; see B&W, p. 1417). As long as the different networks in the populations being compared may be matched between the two social structures (e.g., by setting in correspondence positive and negative choices, respectively, across two sociometric data sets), the blockmodel algebras of the two populations may both be embedded in the congruence lattice of the same free semigroup (B&W, p. 1416; see also Clifford and Preston [1961] for algebraic background). Common membership in this lattice provides a very natural basis for comparing the algebras (and hence also the network data underlying them) with reference to the "length" of the lattice path connecting the two given algebras via their lattice union.² This lattice union, named the "joint reduction" by B&W, is also a semigroup, so that the comparative information derived from the approach is structural and relational as well as metric.

The substantive advantage of this algebraic strategy is that it furnishes a direct method for comparing or contrasting social structures arising in radically different environments (e.g., a management hierarchy vs. a monastery). The social groups compared may be of different sizes and the blockmodels used to describe them may have different numbers of blocks. While implicitly accepting this framework, Bonacich criticizes B&W for their technical handling of several issues in the development of the approach. He makes two main criticisms, on which we comment in reverse order of his presentation.

The first of his objections to be dealt with concerns our preference for basing a comparison of two blockmodel algebras on the path leading through their lattice union (joint reduction) rather than the path through their lattice intersection, which is Bonacich's preferred alternative. Figure 1, adapted from a similar diagram in Boorman and Arabie (1972), gives a rapid initiation into the geometric issue. As this figure suggests, there is considerable formal symmetry between the two alternatives, which is a reflection of general "self-duality" properties of lattices as abstract mathematical structures (see Birkhoff 1967, chap. 1). For this reason, our continuing preference for the original B&W approach over the variant Bonacich proposes is more substantive than formal.

We start with the purely technical observation that the intersection

²For convenience in the following discussion, we will always regard the semigroup algebras used in this approach as interchangeable with the associated congruence relations. Since these relations are (finite) partitions of the denumerably infinite set of words in the free semigroup, the terms "union" and "intersection" will always be used in their partition lattice meanings. See, e.g., Boorman and Arabie (1972).

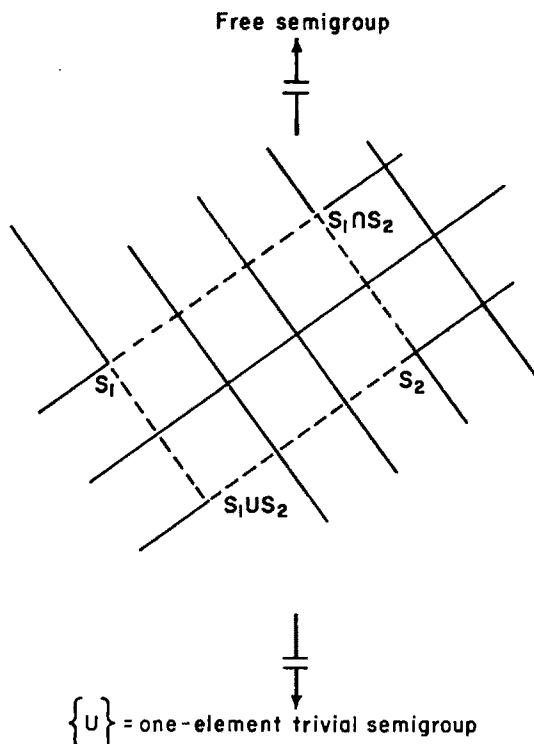


FIG. 1.—Alternative paths between two semigroups in the congruence lattice. $S_1 \cup S_2$ = B&W joint reduction algebra, $S_1 \cap S_2$ = Bonacich alternative (intersection algebra).

algebra will typically be *larger* than either of the data algebras, whereas the B&W joint reduction will be *smaller* than either, often considerably smaller.³ As Bonacich also emphasizes, a central theme of B&W is attention to the substantive sociological interpretability of the algebraic constructions, quite apart from any derived numerical measures of proximity or distance. In particular, B&W placed major emphasis on the multiplication tables of the semigroups interpreted as “role tables” (systems of compound social roles; see p. 1396 and compare the tradition of Nadel [1957] and Lorrain [1975]). From this interpretive standpoint, we find it implausible that the “common structure” of two role systems should, as Bonacich’s proposal would imply, be a *more* elaborate system of roles than either of the two original role systems being compared. Intuitively, one would expect such a “common structure” to be coarser—less differentiated—than either of the initially given structures, if only because complex role frames demand greater and more specialized effort and attention from participants

³ Of course, when one of two algebras being compared is itself a homomorphic reduction of the other, size inequalities will be nonstrict.

than do simpler ones (compare Levy [1952, pp. 383–88], as well as Boyd [1969, pp. 148–58], using kinship data from Lane [1960] and White [1963]). It is certainly true, as Bonacich contends, that such an aggregated concept of “common structure” implies more, not fewer, identities between compound roles. But this is surely no ground for objection. For example, the quite common finding in computing the joint reduction—that most compounds are equated to a universal trivial role (whose associated block-model image is completely filled with ties)—is evidence of simplicity in a sociological sense, not complexity as Bonacich contends in the comment preceding this reply.

To illustrate these points concretely, turn to the Sampson monastery data shown in figure 2. Here three different algebras are being compared, representing respectively the top two and bottom two choices on Sampson’s Esteem relation, $SM(4)-5-E(2)D(2)$; Influence relation, $SM(4)-5-I(2)N(2)$; and Sanction relation, $SM(4)-5-K(2)B(2)$ (five-block model, strict zeroblock criterion used in each case; see B&W, table 1, for notation and other details). The sizes of these algebras are respectively 11, 12, and 7 (see B&W, fig. 15). The joint reduction of these three algebras has been computed using the original B&W algorithms (JNTHOM, MASTERGLB). The Bonacich alternative (intersection of the three algebras) has also been

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>	<u>7</u>	<u>8</u>	<u>9</u>	<u>10</u>	<u>11</u>	<u>12</u>	<u>13</u>	<u>14</u>
<u>1</u>	3	5	7	9	11	13	7	9	9	9	11	14	13	14
<u>2</u>	4	6	8	10	12	14	8	10	9	9	14	14	14	14
<u>3</u>	7	11	7	9	11	13	7	9	9	9	11	14	13	14
<u>4</u>	8	12	8	9	14	14	8	9	9	9	14	14	14	14
<u>5</u>	9	13	9	9	14	14	9	9	9	9	14	14	14	14
<u>6</u>	10	14	10	9	14	14	10	9	9	9	14	14	14	14
<u>7</u>	7	11	7	9	11	13	7	9	9	9	11	14	13	14
(a) <u>8</u>	8	14	8	9	14	14	8	9	9	9	14	14	14	14
<u>9</u>	9	14	9	9	14	14	9	9	9	9	14	14	14	14
<u>10</u>	10	14	10	9	14	14	10	9	9	9	14	14	14	14
<u>11</u>	9	13	9	9	14	14	9	9	9	9	14	14	14	14
<u>12</u>	9	14	9	9	14	14	9	9	9	9	14	14	14	14
<u>13</u>	9	14	9	9	14	14	9	9	9	9	14	14	14	14
<u>14</u>	9	14	9	9	14	14	9	9	9	9	14	14	14	14

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>
<u>1</u>	3	5	3	6	5	6
<u>2</u>	4	6	6	6	6	6
(b) <u>3</u>	3	5	3	6	5	6
<u>4</u>	6	6	6	6	6	6
<u>5</u>	6	6	6	6	6	6
<u>6</u>	6	6	6	6	6	6

	<u>1</u>	<u>3</u>	<u>2</u>	<u>5</u>	<u>4</u>	<u>6</u>
<u>1</u>	3	3	5	5	6	6
<u>3</u>	3	3	5	5	6	6
<u>2</u>	4	6	6	6	6	6
<u>5</u>	6	6	6	6	6	6
<u>4</u>	6	6	6	6	6	6
<u>6</u>	6	6	6	6	6	6

<u>1</u>	1	2	3
<u>2</u>	3	3	3
<u>3</u>	3	3	3

FIG. 2.—Comparison of algebraic constructions based on three Sampson (in press) data sets (for description see text): *a*, Intersection algebra multiplication table; *b*, Union algebra (= B&W joint reduction) multiplication table; *c*, Joint reduction blocked to indicate existence of homomorphic reduction onto T_1 table.

computed, using the algorithm in the appendix to his comment.⁴ The resulting joint reduction and intersection algebras are shown in figure 2. The Bonacich "common structure" algebra (fig. 2a) has 14 elements, so that its multiplication table contains $14^2 = 196$ equations, each of which is potentially interpretable. The B&W joint reduction has just six elements, corresponding to the 6×6 multiplication table shown in figure 2b. In addition to evidencing quite regular structure in its first three rows, this latter multiplication table is also readily blocked as shown, indicating the existence of a further homomorphic reduction down onto the 3×3 table shown in figure 2c. This last table, abbreviated T_1 by B&W (p. 1435), contains only two informative equations ($1*1 = 1$ and $1*2 = 2$) and is the table found by B&W's algebraic analyses to be one of the structural "cores" of the Sampson social organization (see B&W, pp. 1433-34).

In contrast to this essentially simple result, the 14×14 structure in figure 2a seems formidably complex and relatively unenlightening, at least when considered as it stands without any reduction first being applied. There seems little substantive or formal gain from adhering to a structure so complex. Indeed, one suspects that any initial preference for working with the intersection instead of the union of blockmodel algebras may tend to be quixotic in practice, since some sort of reduction will be necessary before substantive interpretation may proceed. (In this regard, note that Bonacich does not actually undertake any new sociological data analyses, using either our approach or his own.) Moreover, note that the concrete example just considered seems quite predisposed toward the Bonacich approach, since the common source of the three underlying data sets suggests that the three algebras being compared will *not* be highly different. Thus, the size of the intersection algebra is modest instead of approaching its theoretical maximum of $11 \times 12 \times 7 = 924$ distinct elements.

These considerations reinforce our continuing preference for working through the B&W joint reduction rather than with Bonacich's alternative approach. However, we also report the results of a further empirical study making a direct comparison between the Bonacich approach and the B&W one as alternative bases for *numerical* (proximity or distance) comparisons of social structures such as those shown in figures 13 and 14 of B&W.

Specifically, we introduce the following structural distance measure based on the Bonacich approach but otherwise intended to be as closely parallel

⁴ It should be noted that the first paragraph in Bonacich's appendix contains a misstatement, since it appears to define the intersection algebra as the algebra generated by taking *all* possible ordered pairs of generators of the two algebras being compared (cf. Coxeter and Moser [1965]). In fact, of course, only ordered pairs of *corresponding* generators should be used, e.g., in Bonacich's example the pairs (p,p) and (n,n) but not (p,n) or (n,p) . This confusion seems to clear up in his next paragraph, however, and the computation there as well as the one in the text of his comment is correct.

as possible to the original (joint-reduction-based) measure δ employed in B&W (see p. 1423):

$$d(S_1, S_2) = \frac{\sum_{c \in P} \binom{|c|}{2}}{\binom{N}{2}} + \frac{\sum_{d \in Q} \binom{|d|}{2}}{\binom{N}{2}},$$

where N = size of intersection algebra S_{LUB} determined by S_1 and S_2 (see B&W, p. 1420, for notation) and

P = partition on elements of S_{LUB} determined by the inverse of the
homomorphic reduction $S_{LUB} \xrightarrow{\phi} S_1$;

Q = partition on elements of S_{LUB} determined by the inverse of the
homomorphic reduction $S_{LUB} \xrightarrow{\psi} S_2$.

(See also B&W, pp. 1420, 1422.)

As in the case of the original measure δ , d will not in general be a metric. However, since as in B&W the primary purpose of d is to create input for multidimensional scaling, violations of the triangle inequality in the input distance matrix are tolerable. Finally, therefore, we applied the multidimensional scaling program *kvst* (Kruskal 1964a, 1964b; Kruskal, Young, and Seery 1977) to a lowerhalf matrix of proximities between all pairs of the 19 cases⁵ from B&W, table 2, using the formula for d given above to define the numerical entries of the matrix. The same scaling options were employed as in B&W (p. 1429), and the best solution from 20 different random initial configurations (Arabie 1973, 1978) is shown in figure 3 (Kruskal's measure of badness-of-fit stress formula one was .1030). This scaling is to be compared with figure 13 in B&W, reproduced here for the reader's convenience as figure 4.

On inspection, it is apparent that the two methods are performing quite similarly, so that at this numerical level of comparison adopting Bonacich's approach does not lead to any major new insight. In particular, both methods acceptably group together the Bank Wiring Room (Roethlisberger-Dickson) cases, the Sampson cases, and each of the two Newcomb cases (although the Year 1 Newcomb cluster is less cohesive in the scaling

⁵ Two errors in the table 2 labeling in B&W should be called to the reader's attention and corrected; BW-2-LA is based on an underlying partition of the actors (W4 S1 W3 W1 I1 W8 W9 W7 S4) (W2 W5 I3 W6 S2) instead of the partition BW-2 indicated in table 1, part IV. Also, NF1(13)-4-L(2)A(2) should be corrected to NF1(13)-4-L(3)A(2). Once these corrections are made the results in fig. 13 of B&W stand unchanged.

based on the Bonacich approach than in the original scaling).⁶ As in the original scaling, the Sampson cluster recognizably separates the two Newcomb ones, although again the pattern is less clear-cut in figure 3 (where the three clusters form essentially an L-shaped configuration) than in figure 4 where a linear arrangement is very clear-cut. In this numerical example,

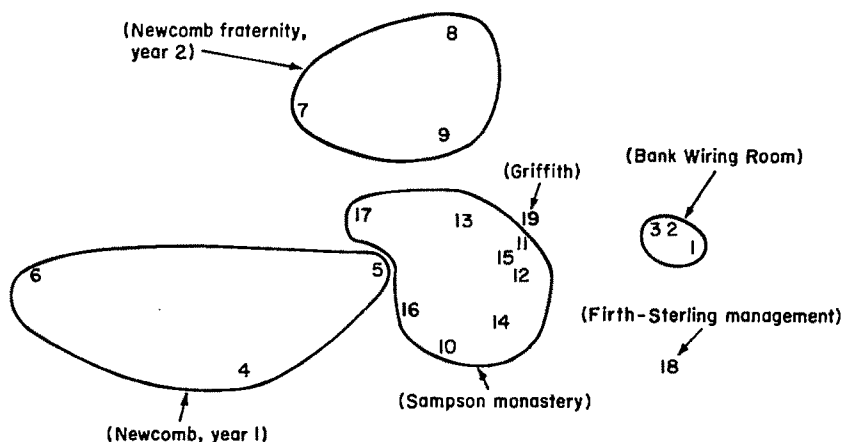


FIG. 3.—Scaling of 19 data sets from B&W, table 2, using the distance measure d based on Bonacich's approach. Two dimensions, Euclidean metric.

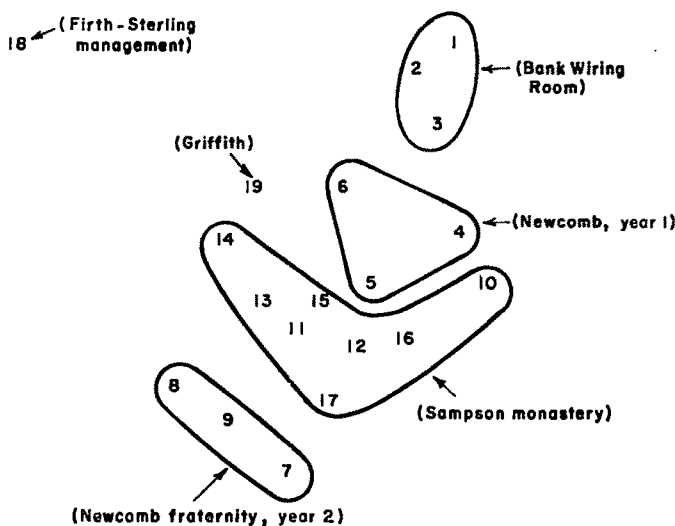


FIG. 4.—Reproduction of fig. 13 from B&W (p. 1429) for purposes of comparison with fig. 3.

⁶ For descriptions of these case studies see, respectively, Roethlisberger and Dickson (1939) and Homans (1950) (Bank Wiring Room group), Sampson (1969) (monastery group), and Newcomb (1961) (two experimental college fraternity groups).

therefore, the only apparent utility of implementing the Bonacich approach is the extent to which it provides a possible check on the robustness of role structure comparisons based on the figure 4 scaling (e.g., to explore caveats such as that voiced in B&W, p. 1432, as to whether the Sampson cases should truly be regarded as intermediate between the two Newcomb years, in view of certain features of the underlying blockmodels noted there). Such a use of alternative structural measurement approaches to provide controls on each other's performance finds support in our earlier comparative studies of different distance measures applied to finite partition lattices, where both union- and intersection-based measures were found to be useful (see Boorman 1970; Boorman and Arabie 1972; Arabie and Boorman 1973; Boorman and Olivier 1973; Day 1979).

Bonacich's other criticism is minor and may be dealt with briefly. Specifically, he notes a case in which the B&W joint reduction fails to be "generator-preserving" (hereafter abbreviated GP) in the sense that distinct generators in the data algebras are forced into equality in the joint reduction algebra (see B&W, p. 1416, for formal definitions). Since this violation of the GP requirement is formally as well as substantively inessential in all the B&W data analyses (all of which may be developed in the congruence lattice of a free semigroup), the violation may be ignored without loss to either the theory or its applications.

Alternatively, if it is desired to stick to the letter of the B&W approach, attention may indeed be limited to GP homomorphisms. Then failure of this requirement (causing the "joint reduction" to be undefined where failure occurs) may be regarded as an informative substantive result, namely, that the algebras being compared are in fact very different—indeed, as different as they can be in the comparative framework adopted. Far from indicating that the B&W machinery is faulty, such "orthogonality" may be a very useful insight about empirical social structures being compared. Note that an alternative approach utilizing Bonacich's intersection algebra yields no such signal of orthogonality, and in many substantive contexts this lack may well be a disadvantage.

SCOTT A. BOORMAN

Yale University

PHIPPS ARABIE

University of Minnesota

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CONCERNING ANOTHER RESPONSE

Another response of some length, focusing more closely on the algebraic issues Bonacich raises and entitled "Equating the 'Joint Reduction' with Blockmodel Common Role Structure: A Reply to McConaghy," has been written by Philippa E. Pattison (*Sociological Methods and Research*, in press).

HARRISON C. WHITE

Harvard University

SEX, SEGMENTS, AND STRATIFICATION: COMMENT ON SNYDER, HAYWARD, AND HUDIS¹

In their recent research note, "The Location of Change in the Sexual Structure of Occupations, 1950-1970" (*AJS* 84 [November 1978]: 706-17), Snyder, Hayward, and Hudis sought to locate changes in the sexual structure of occupations and to explain them in terms of "market segmentation perspectives." Their study contributes to a burgeoning empirical literature which draws on "insights from labor market segmentation theory" (the subtitle of their note) in analyzing occupational inequality. The work of Snyder et al. demonstrates (more persuasively than many previous attempts) the promise of segmentation perspectives for sociological studies of labor force processes. Nonetheless, their research epitomizes the unsettling direction in which such studies have proceeded.

The authors examine changes in occupational sex composition between 1950 and 1970 by relating various specifications of proportionate change in female employment to characteristics summarizing the initial proportion of females and the "skill level" (i.e., specific vocational preparation, or SVP) associated with an occupation, as well as the median income, education, and "steadiness" of employment (i.e., full- vs. part-time) of its incumbents. These measures, they claim, "capture some primary/secondary distinctions" (p. 711), but they concede that their dichotomous formulation of these explanatory variables, utilized throughout the first part of their analysis, "is a conceptual orientation rather than an empirical mandate since no procedure for classifying occupations into one segment or the other has been developed" (p. 711). This statement, coupled with the analytic strategy they adopt, reveals the weakness in their approach: they are predestined to receive few genuine insights from segmentation theory because they remain wedded to a perspective which approaches the underlying na-

¹ I acknowledge gratefully the thoughtful suggestions of William Bielby and an anonymous *AJS* reviewer.

ture of labor stratification in terms of individual characteristics rather than the attributes of (and linkages among) jobs, firms, and industries which are the province of segmentation theory.²

Snyder et al. seem to conflate the existence of a dual economy with the existence of a distribution of workers with certain characteristics that are more or less "typical" of primary versus secondary markets.³ Yet differences in occupation-specific means and variances for such variables as median income and education, median proportion employed full week and/or full year, SVP, and the like may well be *consequences* of a structure of segmented labor markets but are not necessarily *indicators* of it (cf. Beck, Horan, and Tolbert 1978, p. 706).⁴ (In the same manner, some Marxist students of inequality [e.g., Wright and Perrone 1977] might argue that occupational status differences are *generated* by the structure of class relations within production but would certainly not view the former as adequately indicating the latter.)

The authors attempt to tap labor market structure by dichotomizing occupations into those which are "high" on all the "good" things—and thus primary sector occupations—and those "low" on those dimensions (i.e., secondary sector); their principal result is that the greatest temporal variability in occupational composition by gender occurs "within the low categories that crudely represent secondary location" (p. 712). Having thus formulated the problem, Snyder et al. are unable, in my view, to provide a substantive interpretation of this major finding which either convincingly

² For a similarly skeptical view of the emerging "structuralism" in stratification research, see Bielby (in press).

³ As a reviewer noted, one problem associated with attempts to demarcate "sectors" in terms of such attributes as sex is that there is considerable occupation-specific sex segregation and sexual income inequality not only within industries and sectors (e.g., Lester 1967; Hodson 1978; Beck, Horan, and Tolbert 1979; Bibb and Form 1977) but within firms as well (e.g., Bridges and Berk 1974, 1978; Talbert and Bose 1977; Halaby 1979; see also Malkiel and Malkiel 1973). While such findings by no means contradict segmentation approaches, they do suggest the limited utility of such gross conceptualizations and operationalizations of markets or sectors. The confusion occasioned by Snyder et al.'s formulation is further illustrated by references in their longer manuscript (Snyder, Hayward, and Hudis 1978) to "employers in particular occupations" (p. 14; see also p. 22). This phrase makes sense only if labor markets are occupational, a claim typically made by dualists exclusively for certain forms of craft and administrative employment (see Piore 1975). While Snyder et al. seem to imply throughout that "employers" can be characterized by their "occupations," one could construe segmentation perspectives as suggesting the opposite.

⁴ In their longer manuscript (Snyder et al. 1978, p. 13), the authors argue that "it appears reasonable to consider [that] high wages, skill requirements and employment security . . . will characterize stable occupations. These stable categories should also be heavily male, though as a consequence rather than a defining characteristic of primary segment occupations." However, "percent female" is subsequently employed throughout their analyses as an "explanatory variable" serving to represent the distinction between primary and secondary segments (see also 1978, p. 22).

elaborates the logic of segmentation theory or successfully refutes potential alternative explanations.

They argue that primary occupations are stable "because their stringent employment requirements . . . and statistical discrimination provide tight control over entry points" (p. 712).⁵ However, one could easily arrive at other interpretations which are at least equally plausible. For example, human capital theorists might allege that the observed relationship between median income, education, and full-time employment, on the one hand, and the stability of occupational sex composition, on the other, simply expresses the fact that women generally possess less human capital than men do; thus, the former's entry into, movement within, and exit from the labor force is most frequent within the set of occupations whose incumbents are typically "low" on those dimensions (which presumably are proxies for productive capacity).

A different dualist account from that offered by Snyder et al. would emphasize employment instability as a reflection of the short-run attitude typical of periphery *firms* (Averitt 1968). Their limited time horizon stems from restricted market situations and product lines, financial limitations, lack of capacity for planning and technological innovation, and the characteristic organization of periphery production which renders such firms more subject to oscillations of the economy.

Finally, a more radical segmentation view might interpret their findings as evidence of the crucial importance of women within the modern industrial reserve army. Women move in and out of the occupations which the authors classified as "low" because females' class position confines them to secondary forms of employment which serve to cushion the recurring shocks of capitalist development. At the same time, the subordinate class position of women which prevents stable, meaningful, and lucrative employment is only exacerbated by their movement into, out of, and within the labor force, since this promotes, in turn, subsequent statistical discriminations and precludes the development of female worker solidarity and/or "human capital investment."

⁵ Certainly the mechanisms governing labor force entry and exit differ from those affecting labor force *mobility*. Yet the authors do not directly examine whether the greater variability in sex composition within so-called secondary occupations reflects a tendency for women to move in and out of the labor force via the periphery or a tendency to move around from job to job within it. Wolf and Rosenfeld (1978), whose research on occupational sex segregation is also inspired by labor market segmentation theory, have offered evidence pertaining to both mechanisms. They found that "women's" occupations are easier to reenter (for males and females) but furnish few chances for upward mobility. Indeed, if one focused exclusively on occupational shifts within a closed sample or population of workers, segmentation perspectives would presumably suggest the opposite hypothesis from that examined by Snyder et al., namely, that stability should be greater in the secondary sector, which embraces much of "women's work" and provides few opportunities for occupational mobility. Rosenfeld and Sørensen (1979) present results consistent with this claim.

The issue here is not whether one of these explanations is particularly persuasive, but rather that this study allows the debate to remain within the realm of speculation.⁶ Snyder et al.'s trivialization of segmentation theory is even more apparent in the discussion of table 2 (p. 714) which presents regression equations relating the occupational characteristics described above to various specifications of temporal change in the percentage of females in an occupation. The coefficients obtained reveal such findings as these: an increase of \$1,000 in occupation-specific median income is associated with a net decline of 0.56% in the proportion of females in an occupation between 1950 and 1960, and a decline of 0.73% in the absolute value of change over the same decade.⁷ I do not grasp the significance of such results for a segmentation perspective. According to their specification, if these regressors have anything to do with occupational sex segregation, apparently their "effects" correspond to some sort of movement along an interval scale whose "low" point corresponds to the "most secondary" types of occupations and whose highest value represents the "most primary."⁸ Surely this is an unsatisfying representation of labor market segmentation theory, the main message of which seems to be that processes of labor force transformation and stratification must be understood within the context of the organizational and institutional structures which sustain and define those processes.

Dual or segmented labor market theory is not a framework for studying primary versus secondary workers, or even primary versus secondary occupations. Instead, I argue, it is a theory about jobs and firms, and the implications of internal labor markets and economic structure for the organization of work and the relationships among firms within and between industrial sectors. While Snyder et al. allude to this distinction in a footnote (p. 709)—and in greater detail in a longer paper (1978, p. 12)—I think their justification for ignoring a job-level analysis is suspect for several reasons.

⁶ In a footnote (p. 712) Snyder et al. concede their inability to explore competing interpretations (e.g., the differential effects of technological change) because of "data limitations." However, I suspect that the fundamental limitations are conceptual and that the author's failure to achieve a more creative framework for analyzing the relationship between labor market segments and sex segregation is responsible for the empirical shortcomings of the research. (Data pertaining to jobs, firms, and industries, and the allied effects of technological change, *do* exist, but one must have the substantive motivation to go out and find them—see Bielby and Baron [1979].)

⁷ The median education variable is curiously absent from these analyses, and SVP (rather than income) is deleted in a seemingly arbitrary fashion on the grounds that the two are highly colinear.

⁸ In their longer manuscript (1978, p. 18), Snyder et al. deal with this issue in a manner which I find confusing and unsatisfying: "Given that segmentation criteria data are almost invariably continuous, occupations must be described as more or less reflecting primary or secondary sector location. However, for present purposes it does not matter greatly if available measures imply anything more than 'desireability' insofar as a segmentation perspective rests on a given pattern of association with observed gender compositional shifts."

First, occupations simply are not relevant productive (or analytic) units within segmentation theory. Neither establishments nor jobholders think or act in terms of census three-digit occupations, but in terms of the task requirements embodied in the jobs which constitute the firm's division of labor. Second, job characteristics may, in fact, be relatively homogeneous within occupations without necessarily implying that the major locus of change is occupational. Instead, the relationship between job attributes and sex segregation might well change predominantly within occupational categories. Third, Snyder et al. cite Temme's (1975) findings which indicate that between 68.5% and 77.1% of the variation in job-level characteristics exists between occupational categories; indeed, the intraoccupational variance may be even smaller in other data sets (see Snyder et al. 1978, p. 12 and n. 7). However, only one such measure—SVP—figures in their analysis. Whether other job attributes are equally homogeneous is an open empirical question.⁹ Furthermore, there may be appreciable differences between occupations in the patterns and amount of covariation among job characteristics—that is, the ways in which such dimensions of work as requisite training, general educational level, and placement within ladders of supervision and promotion cluster for the jobs in each occupational category. In any event, the fact remains that a job-level characteristic is still a job-level characteristic, regardless of how much of its variance is tapped by some coarser classification.¹⁰ Finally, as Spenner (1977, p. 70) notes, "whether or not [Temme's] figures are high or low depends on the research purpose." Gordon (1972, chap. 5), among others, claims that the industrial labor force is characterized by an increasing proliferation of detailed job categories and increasing intraoccupational heterogeneity. Gordon views both as manifestations of the trend toward deskilling and "hierarchy fetishism" in capitalist development, forces which "attain an independent rationale over and above efficiency rationale" (1972, p. 77). Thus, the degree of heterogeneity within occupations is itself a part of the historical puzzle to be solved by empirical research rather than a convenient assumption to be imposed by fiat.

The differentiation of economic roles must be seen as a reflection of the historical, institutional, and organizational relations posited by segmentation theory. Exploiting the potential insights of a segmentation perspective

⁹ Bridges and Berk (1974, p. 213), for example, report correlations ranging from .08 to .37 between occupational and job characteristics for a limited sample of white-collar employees. They claim that their restricted range of occupations and the characteristic discrepancy between occupational titles and actual work performed in lower-level, highly female white-collar occupations may explain these low correlations. However, one might argue that degraded clerical jobs are among the most internally homogeneous and thus that these correlations are in fact relatively larger than one might expect among all occupations.

¹⁰ This is particularly true of the ratings studied by Temme, which were explicitly defined to apply to *jobs* by the government analysts who gathered those data for the *Dictionary of Occupational Titles* (see U.S. Department of Labor 1972).

requires the development of a framework for linking jobs, firms, and industries to a theoretically inspired sectoral specification (Bielby and Baron 1979).¹¹ It then becomes much easier to locate the requisite empirical data, since one has a clearer idea of what to look for. Fudging with one's theory in order to suit the data in hand is probably, in the long run, an even more serious offense than the opposite practice.

In short, by focusing attention on the structural sources of inequality, rather than its individual manifestations, segmentation theory inspires hope for a more comprehensive understanding of labor force stratification and inequality than has been gleaned from 15 years of status attainment research. Perhaps more important, it offers the possibility of more fruitful collaborations between economists and sociologists investigating related concerns than have characterized previous research. However, these long-run profits cannot be realized without some major intellectual retooling which must begin presently. If Snyder et al. have not yet applied such tools optimally, perhaps they have helped us sketch a blueprint for future efforts, which I await eagerly.¹²

JAMES N. BARON

University of California, Santa Barbara

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¹¹ Although Snyder et al. claim (p. 711) that no scheme for classifying occupations into segments exists, occupational variables have recently been incorporated in an empirical classification of economic "segments" by Freedman (1976), and a scheme which assigns occupations to primary versus secondary labor markets has been developed by Rosenberg (1975) and utilized by Hodson (1978). A number of recent empirical analyses, however, have more fruitfully approximated theoretical "sectors" by classifying industries (e.g., Hodson 1978; Beck et al. 1978; Stinchcombe 1978; Tolbert, Horan, and Beck 1980). Snyder et al. do not seem to consider the possibility that their unit of analysis might be bogging them down.

¹² Snyder et al. have been invited to reply to this comment.—Ed.

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Review Essay: Radicalism and the Cash Nexus

The Philosophy of Money. By Georg Simmel. Translated by Tom Bottomore and David Frisby. London: Routledge & Kegan Paul, 1978. Pp. xiii+512. \$28.00.

Peter A. Lawrence
University of Southampton

Our gratitude must go to all those who have contributed to the translation of Georg Simmel's *Philosophie des Geldes*. At last we have the whole work in English, albeit 78 years after the first appearance of the book in German.

Tom Bottomore and David Frisby are alive to this passage of time; their introduction is strong on detailing the original reception of *Philosophie des Geldes* and replete with the evaluative comments of the contemporary great as well as those of later generations. This introduction includes too a neat section dealing with divergences between Simmel and Marx in the treatment of money and the theory of value. Another strength of the introduction is a highlighting of the aesthetic dimension of Simmel's writing, with the *point de départ* again being the perceptive observations of Simmel's contemporaries. It is not an accident that, alone among the founding fathers of sociology, Simmel also produced monographs on Goethe and Rembrandt; these are not compartmentalized activities.

If the introduction is generally lively in its presentation of the views of others and in making clear what the translators think about what others thought of Simmel's work, it is not so direct or detailed on how the translators themselves esteem *The Philosophy of Money*. Nor, of course, is opinion ever likely to be undivided on the question of the book's importance for us.

There is clearly a major sense in which *The Philosophy of Money* is a disruptive force for the accepted wisdom of modern sociology: it dethrones industrialism as the determining force of modern society. This is perhaps beguiling, since Simmel's characterization of modern society—in *Philosophie des Geldes* and elsewhere—is not, putting it on a broad canvas, inconsistent with that presented by other founding fathers. The key difference is that he is working out not the implications of industrialization but those of the money economy. It may not at first sight be obvious how radical this is.

Simmel, for instance, sees structural differentiation—though he does not use this term—as the concomitant of the money economy. The emergence of the professions, a *recherché* form of the division of labor, occurs within the money economy. This economy is associated with rationalization, calculation, the distancing of man from nature, and the objectification of culture. Even fashion is a by-product of the money economy, offering simultaneously the virtue of conformity and the gratification of distinctive-

ness. Yet all this, and more besides, is quite consistent with the conventional "before and after" paradigm of industrialization (Abrams 1972).

Simmel's position, however, implies a different etiologic time scale. For him the key "event" in the evolution of the modern world is not the Industrial Revolution or, more modestly, the process of Euro-American industrialization in the 19th century. It is not the Enlightenment, or the Renaissance in (most of) its positive aspects. It is the breakup of feudalism, the monetization of seigneurial dues.

The whole question of Simmel's thematic relationship to the sociological enterprise of his day is, indeed, tantalizingly ambiguous. It has been argued, for instance, by Fletcher (1971) and by Lawrence (1976), that he is very much a part of that enterprise. There are three substantial grounds for this point of view. First, Simmel is obviously involved in the characterization of modern society, and in this he is at one with all his contemporaries. Second, he made a contemporary contribution to the development of sociology by providing a legitimation for its separate existence. This he does with the form-versus-content distinction and with the concepts of *Vergesellschaftung* and *Wechselwirkung*, a contribution which was so recognized by contemporaries. Third, however his starting point may have differed from that of, say, Durkheim, Tönnies, or Weber, he too recognized the (newly) precarious nature of social order. His response to the problem of social order is adumbrated at the outset of his career as a sociological writer with the publication of *Über soziale Differenzierung* in 1890. Here he propounds a more all-embracing if less rigorous version of Durkheim's organic society thesis, and it will be noted that this predates the appearance of Durkheim's *Division of Labor in Society* by the better part of a decade. Simmel, in this early work, also explores what he calls the *Prinzip der Kraftersparnis*, the principle of economy or of energy saving (Simmel 1890). This involves him in a discussion of the functional and dysfunctional elements of structural differentiation, which should also be viewed as belonging to his answer to the question of how social order is possible. In all this he is a central figure in the development of sociology.

The essence of the opposite case, the view of Simmel as a lone wolf among the founding fathers, is his rejection of industrialization as the determinant of modern society—the position which is highlighted in *The Philosophy of Money*. Here he not only rejects industrialization as a causal phenomenon; he also slights it in other ways. Consider, for instance, the fact that he contrived to write a 500-page book on the money economy without discussing money as capital or the theory of the firm. Furthermore, at the homely, everyday-life level, there is little about the book which dates it as a product of the late 19th century rather than the 18th century. When Simmel wants an example, an illustration, or an analogy, he is somewhat more likely to draw on his knowledge of ancient Greece or North American Indians than to call forth the factory worker of his native Berlin. This tendency is not restricted to *The Philosophy of Money*. In *Über soziale Differenzierung* the discussion of occupational role specialization owes more to the medieval church than to Bismarck's Germany.

A recent contribution to the debate about the British Industrial Revolution (Fores 1979) has argued inter alia that protagonists of "the revolution" have tended to confound industrialization and urbanization, whereas urbanization in the British case was temporally prior. This line of argument is consistent with Simmel's approach: for him the town is associated with exchange, the money economy, the overstimulation of modern life, and the kind of personal freedom celebrated in *The Philosophy of Money*—a modern version of *Stadtluft macht frei*. This predominant concern with urbanism is yet another manifestation of Simmel's rejection of industrialization. The paratypical phenomenon of Simmel's conceptual world is the metropolis (*Grossstadt*), not the industrial corporation.

Another sense in which Simmel stands somewhat apart in *The Philosophy of Money* consists in the apparent disjunction between the book's theme and his personal experience. One simple aspect of this is that he did not suffer from money problems; the shabby treatment he received from Berlin University was compensated, financially, by the provision made for him by his guardian. *The Philosophy of Money* does not represent the poor man's fascination with wealth or even a sublimation thereof. Yet Simmel's interest in the phenomenon of money is manifest not only in this major work but also in his depiction and analysis elsewhere of what might loosely be called "money figures": the miser, the beggar, the spendthrift, the prostitute, and so on.

A further contribution to the irrelevance of personal experience is Simmel's implied time scale, which precludes an autobiographical realization of the societal transformation documented in *The Philosophy of Money*. One gets a stronger sense of personal awareness of social change in, say, Durkheim, and in the case of Tönnies both the fact and the evaluation of the *Gemeinschaft* and *Gesellschaft* types appear to be predicated on a contrast, in Tönnies's consciousness, between his happy childhood in rural Schleswigland and the perceived features of German society in his mature years. Or again, when Simmel is engaged in characterizing modern society, his views appear to owe little to personal experience. Although he traveled widely in Europe, it seems to have been for cultural rather than, in any sense, sociological purposes. He traveled to admire (and collect) works of art, to visit poets and writers (Laurence 1975), rather than to observe society or extend his social horizons. He tended to depict himself as European rather than German, but this is a cultural identity rather than one reflecting a generalizing social awareness.

We may bring together these two themes—Simmel's rejection of industrialization and the cerebral nature of *The Philosophy of Money*—and ask why Simmel was so interested in money and its implications for social relations. Why did he choose this theme, and why did he make the money economy the determining variable? After all, if one sees *The Philosophy of Money* as a work serving to explain why modern society is as it is, there is some choice in the matter of determining variables.

In any attempt to answer this question, we are handicapped by the fact that there is no biography of Simmel. We lack, that is, a detailed knowledge

of his life and any systematic attempt to relate it to his work. It is surprising how little this simple fact has figured in appraisals of Simmel. But if there can be no definitive answer, it is possible to point to certain compatibilities between the theme, on the one hand, and Simmel's disposition and experience, on the other.

First, the whole operation of *The Philosophy of Money* is nicely compatible with Simmel's lack of involvement in anything approaching empirical research. The theme attracts because it is cerebral, and it is susceptible to a reasoning-and-analogy treatment. "The money economy" has few empirical referents: both theme and treatment suit Simmel's Europeanism, erudition, and self-conscious cleverness.

A variation on this theme is to view Simmel's choice in disciplinary terms. One of the several ways in which he ran afoul of the German academic establishment was in his disinclination to specialize. His catholicity of interest is clear from the range of teaching assignments he undertook (Laurence 1975, pp. 35-36) and, of course, from his wide-ranging publications. He is often described as apolitical and ahistorical; he might be termed adisciplinary as well. *The Philosophy of Money* is a good example: here a sociological question is answered in terms of economic history by means of a philosophic exercise.

Laurence (1975) has emphasized Simmel's generalized disengagement. He was not politically active, he was not concerned with "where Germany was going," and he was not inclined to challenge anti-Semitism (or for that matter the power of the Prussian educational bureaucracy), although he was its victim. We may add too that he was not a pacifist, not a Zionist, not a factory reformer, not even a social critic in the conventional sense. But he did care about European culture and the culture-personality nexus. The objectification of culture, or the increasing gulf between personal and objective culture, is treated in *The Philosophy of Money* and is also a recurrent theme in his later essays. And in the book he published during the First World War (Simmel 1917), the effects of the war on European culture form a major theme. In short, Simmel is attracted to the theme of the money economy because he identifies it as responsible for certain states of mind and social forms tending to produce the cultural malaise which disturbs him.

Finally, if we take the (increasing) industrialization of Europe, and particularly of Germany after 1870, to have been in fact the major development during Simmel's lifetime (1858-1918), it is remarkable how little was his experience of it and the social change it implies. He himself did not experience upward social mobility. He did not make the transition from country to town as Tönnies did, or from small town to metropolis as Durkheim did. Simmel was born in Berlin and stayed there until he was 56. His life was not psychologically uneventful (Laurence 1975), but it was not marked by participation in contemporary social change. And so we have the money economy and its relational implications as the force shaping modern society.

This dethroning of industrialization is really quite spectacular. If the

thesis of *The Philosophy of Money* had become standard, we would, sociologically speaking, have no industrial society. This in turn would mean no postindustrial society, no knowledge-based society, no active society, and perhaps no convergence theory—the true meaning of thinking about the unthinkable.

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Review Essay: From Periphery Back to Center

Annual Review of the Social Sciences of Religion. Vol. 1. Edited by Joachim Matthes, Bryan Wilson, Leo Laeyendecker, Jean Séguy, and Henry Hilhorst. The Hague: Mouton, 1977. Pp. vii+227. \$26.00.

David Martin

London School of Economics

Every schoolman knows that the sociology of religion provided the site on which the great sociologists conducted their classic digs into the nature of culture. Since that time it has often shrunk to an enclave in which a limited section of the academy has studied a limited sector of social life. Differentiation has affected religion and the study of religion alike. Perhaps the prime activity inside this enclave is the study of sectarianism, since the sect by definition stands over against the mainstream and exhibits the typical characteristics of marginality. The distinguished studies of Bryan Wilson in England and Jean Séguy in France are instances of this approach. The sociology of religion becomes affiliated with the sociology of deviance.

This shrinkage is not universal, however. Religion can be linked to all the forms of ultimate concern, to all comprehensive evaluations, and to ideology. Thus religion may be subsumed by the sociology of knowledge and treated within the perspectives of functionalism. Once a functional understanding of religion is accepted, all ideological materials and rituals which shore up the social facade and maintain the social structure become fit subjects for the sociology of religion. Perhaps the prime instance of this is provided by the debate begun by Robert Bellah over civil religion in America which continues the classic work of Herberg (1955). Religion is considered as providing the clothing of the civic sense, and this immediately introduces a comparative and historical perspective. How far is the "secularized" religion of America comparable with the religion of Constantine's Rome, Savonarola's Florence, Louis XIV's France, and Victoria's England?

Apart from these two major trends, the sociology of religion has been mostly a series of footnotes to Max Weber, including the reformulations of the Weber thesis—running from R. H. Tawney to H. Trevor-Roper and from Parsons to Eisenstadt—and all the work on religion as a catalyst of social change in the Third World. These reformulations have activated a renewed concern with the connection between Protestantism and democracy or science as well as with the relation between Methodism and revolution as stated by E. Halévy. Underneath all these concerns, trailing in the wake of Weber and Halévy, lies the issue of secularization, where-

by the ethos of piety succumbs to the spirit of capitalism, or the mobilization of faith provides merely a prologue to the play of political forces. Religion has been depicted as its own gravedigger or as a temporary projection thrown up by more deeply rooted forces and processes.

Such, with distorting brevity, is the context in which the *Annual Review of the Social Sciences of Religion* has appeared. This review is of European provenance and is expressly dedicated to wider theoretical concerns. It begins with a major essay by Thomas Luckmann on secularization and social change and with a penetrating study of confession and social control by Bryan Turner in which Marxist and functionalist approaches are treated as complementary. The theoretical interest in Marxism is underlined by an ingenious article by Séguy in which he constructs a Marxist account of asceticism and, more indirectly, by a case study of the transition from caste to class in Kerala by François Houtart and Geneviève Lemerclinier. Mady Thung expertly exploits points of contact among Christian ethics, ecclesiastical reform, and the reorganization of society. At the broad theoretical level, Wolfgang Lipp contributes a consideration of the social grounding of charisma, and Sharon Siddique applies interactionist theory, more particularly Peter Berger and Luckmann, to material gathered in West Java. Finally, there are two articles bearing on the neglected psychological underside, one by J. P. Deconchy which rightly laments the paucity of the psychological contribution, and the other by Jan van der Lans on experimentation as applied to religious experience, the classic terrain of William James and Rudolf Otto.

This, then, is the scope and thrust of the review. Given that the book is European and theoretical, it provides an opportunity for further exploration of how these themes are being handled today, especially outside America. What is the current state of work, what is the Marxist contribution, and how far has empiricism yielded some ground to symbolic interaction?

The last question is perhaps crucial to what follows. Although the sociology of religion is of all studies the most amenable to the study of meaningful interaction and symbolism, it still illustrates a substantial commitment to empiricism—moreover, to an empiricism that unites Marxist with Catholic approaches and that characterizes Russian as well as American work. Empiricism is widely believed to be dead, but it is a lively ghost. I intend, therefore, to illustrate the kinds of empiricism against which the *Annual Review* is posed and then to consider the points of growth beyond empiricism.

The Marxist study of religion as carried forward in Eastern Europe is a massive enterprise, mostly empirical in form and with a strong practical intent. Eastern European sociologists, sometimes sheltering in institutes of philosophy, study the influence of religion in rural regions and sectors of "limited social relations" in order to bring that influence to an end.

The clearest case of empiricism is provided by Catholic *sociologie religieuse*, which stems from F. Boulard and G. Le Bras and remains very

influential in Catholic Europe and Latin America. *Sociologie religieuse* parallels the remarkable French achievement in historical demography, which has itself included payoffs for the vital statistics of religion. Le Bras, an eminent canonist at the Sorbonne, launched massive inquiries into the historic lineaments of French religious practice and showed how the tone of different milieus was reproduced from generation to generation. This kind of work was taken up by Boulard, E. Poulat, J. Rémy, F. A. Isambert, and others and has created a comprehensive map of French practice over time. It also provides the basis of what is known empirically about religion in Holland (Leo Laeyendecker and Thung), Belgium (K. Dobbelare), Poland (J. Majka, A. Swiecicki), Spain (R. Duocastella, A. Orensanz), Italy (S. Aquaviva), and—by a Lutheran extension—Sweden (B. Gustafsson). It was scholars from this tradition who, with Séguen, formed the nucleus of the Archives de Sociologie des Religions and the International Conference of the Sociology of Religion. The work now is more diffuse and less specifically Catholic as well as less empirical and sociographic in emphasis. For example, the recent meetings of the international conference have been concerned with symbolism (1977) and politics (1979).

The return of the sociology of religion to the larger concerns of sociology is suggested by a convergence with political science. This convergence includes several strands. There is an interest in the extent to which political ideologies like Marxism and nationalism can provide functional analogues to religion. Then there is a complementary Marxist interest in the capacity of religion to draw off political energies or to provide an initial mobilization of emergent groups. There is an overlap here with the studies of millennialism, culminating in Wilson's *Magic and the Millennium* (1973). Research has also focused concern on the role of religion in conducting revolutionary or disoriented youth back toward normality—for example, James Richardson's work on the Jesus Movement. Above all, there is a concern with the impact of the religious variable not merely as affecting voting patterns in certain types of polity but also as creating the ground within which particular political cultures and structures are able to survive and flourish. Seymour Martin Lipset, Stein Rokkan, and Richard Rose have laid the foundations for comparative analyses of religion and politics. (I have attempted a prolegomenon to this enterprise in Martin [1978].)

One element in the political dimension of religion is the connection between religion and class as studied, for example, by N. J. Demerath, Charles Glock, and Rodney Stark in the United States, and by French, Belgian, Spanish, and Dutch practitioners. Some of these scholars are in the tradition of *sociologie religieuse*, for example, Laeyendecker, Dobbelare, Duocastella, Isambert, Poulat, and Emile Pin. Another aspect concerns the role of religion in sustaining national subcultures and cultural defense. Cultural defense has been studied by Rokkan in Norway and Roy Wallis in Britain. Joseph Gusfield initiated one variant of such studies

under the conceptual umbrella of status defense, and this has been taken up by others, for example, Zurcher and Kirkpatrick (1976). The defense of subcultures is linked to the general maintenance of tradition and all the varied and rich relations of center and periphery. In this connection, sociologists of religion can draw on the magisterial work of Edward Shils.

A further and crucial element is the theme of differentiation in relation to religion, politics, and other sectors. Here Talcott Parsons has laid down strong foundations. Daniel Bell's stimulating and controversial *The Cultural Contradictions of Capitalism* (1976) takes further the analyses of disjunction between different societal spheres, including religion. Indeed, the pathbreaking essays by Bellah on religious evolution and civil religion can be considered as studies in differentiation and in what has been retained from less differentiated social formations.

The underlying point is the way religion has engaged the attention of major theorists: Parsons, Bellah, Shils, Lipset, Bell. Religion has begun to acquire again the central interest it had for classical theory. Two important works in the field of theoretical issues should perhaps be mentioned at this point: Robertson's *The Sociological Interpretation of Religion* (1970) and Yinger's *The Scientific Study of Religion* (1971).

The new breed of sociological historians has provided backing for the shift of emphasis just noted. A signal instance is Thomas's *Religion and the Decline of Magic* (1971), in which rich historical materials are woven around frames provided by anthropology. Many historians have returned to the Halévy thesis on the relation of Methodism to revolution. Some, such as Eric Hobsbawm, have controverted the thesis; others have reformulated it—for example, Semmel (1973), Moore (1974), and Thompson (1963). Whatever the precise truth about Halévy's thesis, evangelical religion clearly played an important role in initial mobilizations of consciousness—moreover, a role paralleled today by Pentecostalism in Brazil and Chile and even by the New Religions of Japan. Other related aspects concern the relationship between religion and the emergence of democracy, voluntary associations, and education. There is also the relationship of religion to the development of capitalist agriculture and the new cosmopolis. One might mention here the recent work of three young historians: Stephen Yeo on religion and voluntary associations in the 19th century, Hugh McLeod on class and religion in the late Victorian city, and James Obelkevich on religion in Victorian rural society.

The work of British historians bears some connection to the French school of the historical sociology and demography of religion, but the work of British and French anthropologists has been widely divergent. The functionalism of Malinowski and Radcliffe-Brown had been the progenitor of a distinguished following, for example, E. Evans-Pritchard on Azande witchcraft and on Nuer religion. Witchcraft has interested not only historians but also all those concerned with the nature of rationality and the difference between modern science and primitive conceptions, such as Ian Jarvie, John Skorupski, Robin Horton, Peter Winch, and Ernest Gellner.

Perhaps the most influential anthropologists have focused on the basis and nature of symbols: Mary Douglas, Victor Turner, and Clifford Geertz. Symbols and myths have likewise formed the theoretical focus of French structuralist anthropology. This focus on symbols, shared by such disparate schools of anthropology, indicates a new orientation. One might add that there is one sphere where French anthropology and British anthropology are much more alike: the study of "transitional" religious phenomena in Africa (e.g., Peel 1968).

A field like religion appears to invite the entry of symbolic interactionism. In fact, the main influence has been Schutz mediated by Berger and Luckmann. Both Berger and Luckmann have also exercised a major influence in their own right. Thus, if one were to take (say) the excellent British surveys of the field over the past few years by Michael Hill, Betty Scharf, Susan Budd, and Robert Towler, one would be struck by the prominence accorded Berger and Luckmann. Both men are interpretative sociologists, philosophically and theologically equipped, who recognize the symbolic character of religion and how it maintains the plausibility of social cosmoi and nomoi, thereby holding chaos and anomie at bay.

So the trend is clear: a renewed concern with wide-ranging theory and comparison, philosophical and theological questioning, a convergence with anthropology and political science, and a focus on the nature of symbolization and mythical expression.

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Lewis A. Coser

Center for Advanced Study in the Behavioral Sciences, and State University of New York at Stony Brook

Norbert Elias's *What Is Sociology?* was originally published in Germany in 1970 as a short volume introducing "key problems in sociology." At that time the author, who was born in 1897, had retired from his teaching position at the University of Leicester and was hardly known among British, American, or Continental sociologists—except for some devoted former students. Few of his previous writings were in print, and one senses in reading the book that Elias felt somewhat disappointed by what then looked like a largely unsuccessful career. Shortly after the German publication of this book, Elias was suddenly "discovered" by Dutch, German, and French sociologists. His magisterial work *The Civilizing Process*, first published in 1939, was translated into French in the seventies; the paperback publication of the book by a leading German publishing house was an immense success; he received the Theodor Adorno prize of the city of Frankfurt; and the recent publication of the first volume of *The Civilizing Process* in America has made his name widely known in this country. After many years spent in relative obscurity, Elias, in his seventies and early eighties, was suddenly propelled into a commanding position in the world of sociology.

The fact that *What Is Sociology?* appeared in the original before Elias's recent recognition goes a long way toward explaining the character of the book under review. One gathers from it that at the time of its writing Elias was a bitter man, suffering from the neglect of his colleagues and desperately trying to assert his independence and claim to major stature. He had published earlier (with J. Scotson) a book entitled *The Established and the Outsiders* in which he tried to develop a sociology of the insider-outsider dialectic. The present volume, or so it seems to me, can best be understood as the work of an outsider battling what he perceives to be the establishment of sociology.

I defer to no one in my admiration for Elias's major work—including

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his important *The Courtly Society*, which still awaits translation into English. But I am compelled to state that I consider the present book a failure. Given the textbook character of the series in which it originally appeared, Elias was forced to address some of the perennial problems of sociology. So he deals here, *inter alia*, with the relations between the individual and society, the relative autonomy of sociology as a science, the role of the sociologist as a destroyer of myths, human interdependences and social bonds, the universal features of human societies, and the theory of social development. But, as distinct for example from Peter Berger's *Invitation to Sociology* (Garden City, N.Y.: Doubleday, 1963), which has somewhat similar aims but manages to deal with the perennial questions with a great deal of sophistication and stylistic brilliance, Elias's book hardly leaves the sociological commonplace. Much of what he has to say here can be found in any humdrum introductory text.

Unfortunately, Elias also felt impelled to establish too emphatically the originality of his approach. He made a dogged effort to show that what he had to say had remained unrecognized by his predecessors and contemporaries. Take, for example, the term "figuration," which he considers the key to his method because only approaching sociology in terms of "figurations" prevents the sociologist "from thinking of people as individuals at the same time as thinking of them as societies" (p. 129). But what is gained by a new term that covers a subject matter that has been the birth-right of sociology since the days of Comte or Cooley? Elias tends to ram in open doors.

What is even more grating, however, is Elias's unjustified attacks upon and *obiter dicta* about "established sociologists." A few examples will suffice. The late Talcott Parsons is said "to have taken it for granted . . . that human personality structure is independent of social structure" and "to take the privacy and individuality of every person's bodily sensations as evidence that man is by nature in effect a self-contained and solitary being" (p. 135). Max Weber stands accused of having thought in terms of unilinear causal explanations and of maintaining that "Protestantism was the cause, and capitalism the effect," instead of seeing the two notions as part of a figuration. Robert K. Merton's concept of dysfunctions is said to rest on "an ideal image of harmoniously functioning static societies—an image which does not correspond to anything observable in real life" (p. 177).

Elias's patent misunderstandings and misinterpretations and cantankerous flailing about are an embarrassment, especially to those who, like myself, largely admire his major work. While *The Civilizing Process*, though written in the thirties, reads as if it were written in the seventies, this book, though written in the sixties, reads as if it had been written in the thirties.

There is no way to do justice to the second book under review, the festschrift for Elias that was edited by Dutch and German colleagues on the occasion of his eightieth birthday. *Human Figurations* contains 27 contributions, most of them in English, by as many authors from Holland, England, and Germany. As is to be expected in such a collection, the papers

vary a great deal in topics as well as in quality. I can only mention a few that I consider to have outstanding merit.

Johan Goudsblom's "Responses to Norbert Elias's Work in England, Germany, the Netherlands and France" is an indispensable bibliographical guide for tracing the complicated path of Elias's initial rejection and subsequent "rediscovery" in these countries. Eric Dunning's "Power and Authority in the Public Schools (1700-1850)" furnishes a brilliant interpretation of the changes of authority structures in British public schools in terms of the shifting class positions of the students. Modes of harsh control, he shows, that were thought to be appropriate for the largely bourgeois students after the middle of the 19th century could not be used at earlier times, when most students stemmed from the socially superior gentry. Hence the "unruliness" of the students in this earlier period. W. Baldamus's "Ludwig Fleck and the Development of the Sociology of Science" provides a fine treatment of the hitherto largely unknown contributions to the sociology of science by the Polish physician Ludwig Fleck. He developed the notion of the "scientific community," which is central to the modern sociology of science. Thomas Kuhn has testified to the influence of Fleck on his own work, and Robert K. Merton, I understand, is at work on an edition of Fleck's sociological work. Abram de Swan furnishes a fine description of the social context of psychoanalytical practice in Freud's days in his "On the Socio-Genesis of the Psychoanalytical Setting." A group of papers in historical sociology by H. G. Koenigsberger, Volker Krumrey, and Pieter Spierenburg are excellent contributions to this field. Richard Brown's "The Growth of Industrial Bureaucracy" ably challenges some of the sacred cows in the sociology of organizations. There are also some engaging reminiscences about Elias from friends and former students, among them an especially revealing memoir by the anthropologist Eric R. Wolf.

Although this book also contains, as is almost inevitable, a number of weak contributions, as a whole it is a fit tribute to the man it seeks to honor.

The Essential Tension: Selected Studies in Scientific Tradition and Change. By Thomas S. Kuhn. Chicago: University of Chicago Press, 1977. Pp. xxiii+366. \$18.50.

Thomas F. Gieryn
Indiana University

A profound book is perhaps most easily recognized by the high frequency of its misinterpretation. Thomas S. Kuhn's *The Structure of Scientific Revolutions* has provoked more than the usual response from scholars in every corner of academe, but together the discussions appear like a map of misreading. Kuhn is rightfully proud of the attention given his book,

but he is understandably dismayed at the inattention given his ideas: "I have sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume" (p. 293).

The Essential Tension is essential reading for those who would understand Kuhn, paradigms, and scientific revolutions. Only two of the 14 essays are previously unpublished. This does not at all diminish the value of the collection, for here we have Kuhn in a 1959 essay groping for the ideas that were to become better documented in *The Structure of Scientific Revolutions* alongside Kuhn in a 1976 article still trying to clarify those ideas to errant readers and, perhaps, to himself. The chapters fall into two uneven piles: the first six studies are "historiographic" and the last eight are "metahistorical." That distinction may be lost on a sociologist, but clearly most of the essays in part 1 *do* the history of science, while most in part 2 tell *how* to do it. An inevitable redundancy makes several chapters stale (and cross-references are not what they should be), but reading the same argument two or three times is hardly a bother for those of us who have often pondered Kuhn's earlier books.

The essential tension between tradition and innovation in science, between received wisdom and its necessary corrections, provides the clasp which holds the essays together. A second tension in science and scholarship is that between the intent of an author and the interpretations by readers. Kuhn's substantive focus in this collection is the first tension, but his practical accomplishment is to clarify the unavoidable misreadings that emerge from the second.

The first misreading portrays Kuhn as a thoroughgoing relativist for whom truth is merely mob psychology and objectivity merely ideology. Dudley Shapere reads in Kuhn: "the decision of a scientific group to adopt a new paradigm cannot be based on good reasons of any kind, factual or otherwise" (reported on p. 321). Kuhn's reply is certain: "Reports of this sort manifest total misunderstanding" (p. 321). His elaboration is found in the 1973 Machette Lecture at Furman University, entitled "Objectivity, Value Judgments and Theory Choices," to my mind the most important chapter of the book. Kuhn provides a list of criteria which scientists commonly use to decide among competing theories (among them accuracy, factuality, parsimony, and fruitfulness), and these shared criteria provide science with some of its objectivity. He admits, however, that two scientists committed to these criteria might still reach different conclusions, in part because of their distinctive social or psychological characteristics (such as the length of time that each had employed one of the theories). But the objectivity of the natural world tempers these subjective factors, as Kuhn suggests in an essay entitled "Energy Conservation as an Example of Simultaneous Discovery": "Energy is conserved; nature behaves that way. But we do not know why these elements [of the theory] suddenly became accessible and recognizable" (p. 72). Kuhn's focus on social and psychological factors in theory choice is designed to correct a myopic philosophy of science that has for too long seen the development of science as merely logical: "every individual choice between competing theories

depends on a mixture of objective and subjective factors, or of shared and individual criteria. . . . No standards of factuality or actuality are being set aside" (pp. 325, 337).

The second misreading ascribes to Kuhn a theory of "cognitive primacy" in which scientific knowledge and technique constrain the behavior of scientists, but social or behavioral norms and values do not. British sociologists Barnes and Dolby are mentioned (p. xxi) as among those who would like to drive a wedge between Kuhn's analysis of the cognitive content of science and Robert Merton's analysis of such social values of science as "organized scepticism" and "universalism." Kuhn's reply: "The papers [in *The Essential Tension*] indicate how seriously misdirected I take that line of criticism to be. My own work has been little concerned with . . . scientific values, but it has from the start presumed their existence and role" (pp. xxi-xxii). One might expect Merton to say much the same about the role of scientific knowledge in constraining the behavior of scientists.

The third misreading pictures Kuhn as an "internalist" historian of science whose explanation of scientific change denies the importance of social and intellectual conditions external to scientific institutions. Kuhn is amazed by the charge: "My book has little to say about such external influence, but it ought not to be read as denying their existence" (p. xv). Indeed, Kuhn offers a hypothesis that has yet to receive its due sociological attention: as problem areas in science mature, they become increasingly insulated from priorities of nonscientific institutions. The internalist orientation to *modern* science has been so successful, says Kuhn, because most scientific problem areas have reached the point where problem choice and theory choice are less affected by economic and philosophical factors than "by an internal challenge to increase the scope and precision of the fit between existing theory and nature" (p. 119).

A common theme in these misreadings is captured by the word "merely." Theory choice is *merely* subjective; the behavior of scientists is *merely* the result of the normative force of paradigms; scientific change is *merely* the internal progress of ideas excised from their historical contexts. Kuhn's decision to restrict himself to only a few aspects of science (perhaps for lack of infinite time, wisdom, and curiosity) is misread as a theoretical commitment to exclude unexamined factors as irrelevant to an understanding of science. Kuhn's few words on Merton's ethos of science, for example, cannot be taken as a commitment to a theory that social values have no place in the analysis of the behavior of scientists.

Not all of the confusion over "what Kuhn really said" is due to misreading. Some of the ambiguity is Kuhn's, and much of it is centered on the slippery concept "paradigm." The promiscuous use of paradigm (some would call it "trendy") has decreased its meaning, and several essays in *The Essential Tension* should restore Kuhn's usage. Kuhn would like paradigm to refer to those standard exemplary problems and their solutions which serve to socialize scientists to the research norms of a discipline. If Kuhn had his way, the more encompassing set of widely shared

tacit assumptions that guide research would be called a "disciplinary matrix," which includes not only paradigms (i.e., exemplars) but symbolic generalizations and implicit models of nature as well. It is not only to be philologically correct that Kuhn wants to reserve paradigm for exemplars. A large part of his debate with Karl Popper centers on Kuhn's contention that science is learned and practiced by example—reworking the classic solutions to classic problems—and not by the mechanical application of a set of precise logical rules to the natural world.

The best of Kuhn may be found not in abstract discussions of paradigms but in the analysis of concrete historical events. A 1976 essay "Mathematical versus Experimental Traditions in the Development of Physical Science" asks us to put on new thinking caps to reexamine the scientific revolution of the 17th century. Just what was revolutionary? Some received wisdom suggests that science was thoroughly transformed by the introduction of inductivism and experimental methods to decide matters of truth. Not quite so, says Kuhn. The sciences must first be divided into two kinds. For the *classical* physical sciences (astronomy, harmonics, mathematics, optics, and statics), effects of the new experimentation on conceptual development were minimal. For example, Newton's experiments with prisms to examine the "celebrated phenomena of color" did not break through to new theoretical ground; rather, they helped to consolidate what was already known. In general, experimental measurement is a "mopping-up operation" (p. 188) that provides an "explicit demonstration of a *previously implicit* agreement between theory and the world" (p. 192). The novel contribution of experimentalism was the creation of a new set of specialties, the *Baconian* sciences of magnetism, electricity, and chemistry. Interestingly enough, although these sciences owe their status as sciences to the successes of experimentation, they did not achieve a coherent body of theoretical generalizations until the 19th century, after the quantitative work of Fourier, Clausius, Kelvin, and Maxwell. The different histories of the classical and Baconian sciences illustrate a common pattern: "the road from scientific law to scientific measurement can rarely be traveled in the reverse" (p. 219).

A long preface provides a sparkling introduction to the collection. With a rare kind of critical reflexivity, Kuhn offers an informative biographical context for his ideas. We learn, for example, that the word "hermeneutic" was not part of his vocabulary five years ago, although he now uses the word to describe his method for unpacking the contents of a scientific text: "Look first for the absurdities in the text and ask yourself how a sensible person could have written them" (p. xii). The advice is sound not only for historians looking for useful information in dusty scientific manuscripts but also for sociologists who sometimes too easily find absurdities in the writings of their peers.

Sociology and the Twilight of Man: Homocentrism and Discourse in Sociological Theory. By Charles C. Lemert. Carbondale: Southern Illinois University Press, 1979. Pp. xvi+260. \$17.50.

Don Martindale

University of Minnesota

Charles C. Lemert's eschatological sermon rests, according to the Southern Illinois University Press release, on "the death of man as both intellectual conclusion and fact." Lemert is no new version of Friedrich Nietzsche's madman, who appeared with a lighted lantern in the marketplace at mid-day, reporting this time that "Like God, Man is dead." Lemert takes this for granted, making it the basis for his reluctance to employ personal pronouns: "First person pronouns (I, my, we, our and so forth) have been omitted out of theoretical conviction. . . . One can hardly write a book on the decline of homocentrism in first person" (p. xi). Lemert has a point; it makes no sense at all to use "I" in a world where there is no "I," "you," "he," "she," or "they." Lemert apologizes for his occasional lapse into the first person on the grounds that he has not decided "whether to cry or rejoice at the death of man" (p. xii).

Sociology and the Twilight of Man is based on the gospel according to Foucault. "From within language experienced and traversed as language, in the play of its possibilities extended to their furthest point, what emerges is that man has 'come to an end,' and that, by reaching the summit of all possible speech, he arrives not at the very heart of himself but at the brink of that which limits him; in that region where death prowls, where thought is extinguished, where the promise of the origin interminably recedes" (Michel Foucault, *The Order of Things* [New York: Vintage, 1973], p. 383).

Lemert and his fellow mystics seem to have arrived at their revelation by the following steps. At the end of the 18th century Kant undertook to resolve various contradictions in the conceptual problems of the day by examining the nature and limitations, if any, of thought. Kant came to the conclusion that morality and religion are, in principle, outside the sphere of scientific thought. Moreover, his review of pure reason (scientific thought) led him to the conclusion that even some features of empirical thought were mind given or a priori. At the beginning of the 20th century Russell and Whitehead finally developed a language at once adequate to both logic and science. Wittgenstein then resumed Kant's endeavor, to explore what could be legitimately said about the world, but now not from the standpoint of the structural limits of thought or mind but from the limits of language. In the *Tractatus* Wittgenstein concluded that, after everything that could be said within the limits of language had been said, it turned out to be trivial and everything important remained. Wittgenstein then decided to return the object of science, "fact," to ordinary language, along with the objects of morality and aesthetics, and to try

again to explore what could be said. His unfinished explorations were eventually reported in the *Philosophical Investigations*.

One general trend in philosophical analysis since the 18th century appears in a shift in the focus of analysis from nature to thought about nature and from thought about thought to the language in which thought is expressed. The reasoning of Lemert and his fellow mystics seems to be: if the world, the object of thought, is gradually sinking, surely Man, captain of the ship of thought, must be going down with his ship. And since thought has already disappeared into language, only a sea of talk remains.

Lemert's message is that sociological theory has already vanished into language. He endeavors to prove this by arguing that current sociological theories can be reduced to three types: lexical theories, the sole significance of which consists in seeking to establish dictionaries of words (Talcott Parsons, George Homans, Hubert Blalock, and James Coleman); semantic theories concerned with meanings (G. H. Mead, Herbert Blumer, Aaron Cicourel, Harold Garfinkel, Peter Berger, Thomas Luckmann, and Alfred Schutz); and syntactic theories seeking to ascertain the norms and rules of interaction and communication (Jürgen Habermas, Alvin Gouldner, and C. Wright Mills). For all their apparent differences, Lemert insists, these theories come down to the same thing: individual men are assumed to be trying to define words, ascertain meanings, and discover norms and rules. However: "Without man, lexicality would have no source for its terms. Without man, semanticality would have no consciousness from which to generate meanings. Without man, syntacticality would have no telos of freedom from which to derive rules and norms" (pp. 226-27). Since man is dead, Lemert finds it "reasonable to imagine a relativistic, decentered manless world of talkers talking about talk and texts written about texts."

Contemporary sociology owes a debt of gratitude to Lemert; to Herman R. Lantz, his editor; and to the Southern Illinois University Press, his publisher, for the most elaborate hoax of 1979—complete nonsense disguised as a critique of sociological theory. In these troubled times of inflation, unemployment, depression, energy shortage, and a crisis of confidence in government, we can use a little humor. Surely the proper response to the Alice-in-Wonderland world of Lemert is a smile: preferably like the smile on the face of the Cheshire cat who gradually vanished until only his smile remained. As a matter of fact, Lemert's book deserves a whole world of catless smiles, grinning broadly at other catless smiles.

The Derelicts of Company K: A Sociological Study of Demoralization. By Tamotsu Shibutani. Berkeley and Los Angeles: University of California Press, 1978. Pp. xv+455. \$14.95.

Kurt Lang

State University of New York at Stony Brook

"Derelicts" is a euphemism for the far more colorful epithet by which soldiers usually designate the ineffectives among themselves. *The Derelicts of Company K* is an account of an all-Nisei unit based on a diary kept by one of the participants. What makes such a document a sociological study of unusual interest?

One obvious answer is the analytic framework that guided the selection and organization of material. In addition, one is struck by the way in which the author, Tamotsu Shibutani, has kept himself out of the account. His personal feelings remain muted, and this may be why it took him some 30 years to put into print what must have been a most unsettling experience. The write-up exhibits a commendable commitment to impersonality and objectivity, which is one of the reasons it can be read on several levels—as a painstakingly detailed depiction of the trials and tribulations of the life of enlisted men, as a study in ethnic relations, and as a case study of demoralization as a social process.

The many indignities and irritations suffered by the members of Company K, and also by Tamotsu Shibutani, are recorded in considerable detail, together with the scatological language through which soldiers are apt to vent their anger and the ingenious ways in which they try to maintain at least minimal control over their situation. It is a contest, but hardly a fair one, between military authority and informal group norms. The latter win some skirmishes, but the military wins in the end.

In dwelling on the kind of experiences familiar to World War II veterans who were forced to mark time in overseas replacement depots or on occupation duty in defeated Germany or Japan while everyone else was being demobilized, Shibutani tells you everything you may have wanted to know about the enlisted man's army but that *The American Soldier* volumes cannot tell you even if you were not afraid to ask. Still, this vivid and authentic account is told from a particular perspective, which makes it important to separate what is and what is not "typical."

For one thing, the subjects of this book never got closer to combat than Fort Meade, Maryland, a port of embarkation for Europe. There, while waiting, they were kept busy with the usual details and training exercises. Unlike their Caucasian peers who went over as replacements, Nisei could be assigned only to the deservedly famous 442d Infantry combat team, the Nisei regiment that earned universal respect for its courage in the face of strong enemy opposition. But by the spring of 1945, the regiment was positioned in a quiet sector and incurring few casualties. Indirectly, it was their ethnicity that caused the men of Company K to become supernumeraries.

The stay at Fort Meade was doubly frustrating. When military activity and, especially, training exercises lose their supposed utility, the enforcement of discipline always begins to appear as harassment. Inconsiderate and nonunderstanding leaders enhanced this feeling, especially since many Nisei were eager to prove themselves equal in competence and loyalty to other Americans. They had a special incentive not to waste their skills in useless activity. Also, it seems that whatever resentment they may have felt against the government's relocation of Nisei residents and other unequal treatment was channeled against military authority, particularly against officers and NCOs who perpetuated the unfairness or were intent only on covering up their own mistakes.

The unit soon developed a distinct tradition of negativism. The same factors that had motivated the extraordinary performance of the 442d caused this group, or anyone who joined it, to become intractable and to deliberately reduce its performance in the language school to which it was sent after the need for infantry replacements had ceased. The fact that the members of Company K were kept together as a more or less segregated unit, not totally but enough to preserve a distinctive group culture, also enabled them to deal with the frustrations of the replacement depot in Japan in ways that individual transients, whose stay was short, could not. The extent of collusion in circumventing regulations was more like that of a unit exposed to risk than of a group of interpreters.

This brings us to another topic: demoralization as a social process. Shibutani demonstrates most convincingly that the poor performance of Company K was not a matter of psychiatric disability or lack of occupational skill but a collective coping with the various frustrating elements in its situation. In fact, there were occasions on which these same derelicts performed outstandingly simply because there was a good reason to do so, as when group interests they sought to protect were clearly advanced by such a performance. The point is important and can help us understand other cases of collective indiscipline, such as those among black sailors on the *Kitty Hawk* carrier, within the German fleet at Kiel, in the French trenches in 1917, and those reported from Vietnam. It might also clarify why American troops there whose morale Moskos judged so positively later became rather derelict in their performance of duties, even though the strategic situation had improved.

One should, on the other hand, resist the temptation to overgeneralize from a single case, one of the main dangers of the case-study approach. Although the ineffectual transactions among these Nisei troops did not result from individual alienation but emerged only because inept performances received social support, there are circumstances in which deviant tendencies are the result of individual malaise and weak group ties. Most analysts have erred in the opposite direction. In making interpersonal cohesion synonymous with organizational effectiveness, they have tended to overlook the importance of informal group sanctions in breakdowns. Against this view, there can be no better corrective than Shibutani's step-by-step analysis of the social process of demoralization. But the book is also a con-

tribution in its own right, to be recommended to anyone interested in the military, the interethnic, and the collective-behavior aspects of the story.

Military Institutions and Coercion in the Developing Nations. By Morris Janowitz. Chicago: University of Chicago Press, 1977. Pp. xiii+211. \$12.50.

Gwyn Harries-Jenkins
University of Hull

In his earlier monograph *The Military in the Political Development of New Nations* (Chicago: University of Chicago Press, 1964), Morris Janowitz presented a stimulating and perceptive hypothetical framework which has served as the basis of considerable further research in this controversial area. Now, more than a decade later, Janowitz has extended his original essay to reexamine the hypotheses which were originally put forward. This expansion has been achieved through reprinting the first essay and augmenting it with a new one that seeks to address the wider issues of the changing patterns of coercion in these nation-states. To meet this aim, Janowitz has focused his attention on the effects of an expansion of paramilitary forces which he sees as playing an increasingly important role in a drift toward enhanced authoritarian rule in the developing nations.

Janowitz, Distinguished Service Professor in the Department of Sociology at the University of Chicago and chairman of the Inter-University Seminar on Armed Forces and Society, has always been an original and exciting analyst. His valuable insights and observations in the field of comparative macrosociology have consistently stimulated further research and inquiry. Almost inevitably his work, particularly that concerned with the study of the complex relationship of the military with its parent society, has evoked emotive reactions. Nowhere is this more likely to occur than in response to a volume such as this, where Janowitz has set out to analyze critically the linkages in the developing nations among modernization, political stability, and such coercive institutions as military and paramilitary forces. Each of these themes invites an emotional response. In combination, they test to the full the ability of social scientists to explore the topic of the military in developing nations with a degree of detachment and self-criticism.

In a sense, Janowitz can be said to have deliberately fueled these emotive reactions. On the one hand, he has presented in this volume a set of provocative conclusions. He argues (p. 5) that since 1965 one of the most noteworthy developments in the new nations has been the rapid growth of paramilitary forces that have, over the short run, increased the regimes' stability. Subsequently, he points out that "coercive practices have not only become more frequent and widespread, but they have also frequently emerged as *more effective* ways to consolidate and maintain regime stabil-

ity" (p. 20; my italics). One immediate—albeit erroneous—reaction is that Janowitz is somehow condoning the worst excesses of these paramilitary formations. On the other hand, in presenting data to illustrate the relationship among internal political developments, the growth of the paramilitary units, and the patterns of coercion, he refrains from either condemning or approving such growth. Again, this invites the criticism that a neutral standpoint ignores the dysfunctional consequences of such expansion of paramilitary forces.

What such reactions overlook, however, is the basic thrust of Janowitz's theme. As a concomitant of his wider interest in the sociology of political development, he has begun from the premise that instruments of repressive control are utilized by elites who manage them to implement their goals, aspirations, and organizational effectiveness. This theme emphasizes that, in evaluating the role of the military in the political development of new nations, consideration has to be given to the importance of the part played by the police and paramilitary, who are under the direction and control of the central national military establishment. This is not to condone their use. The inherent limitations on the capacity of a military regime for political rule and the preference for the transformation of these regimes into civilian-based political institutions are readily acknowledged. The reality, however, it is argued, is that, in the absence of or the failure to develop more effective patterns of political and social control, military-based regimes tend to rely heavily on internal police control to consolidate their position.

The central theme of *Military Institutions and Coercion in the Developing Nations* is one that cannot be ignored. Janowitz's persistent concern with the role of the military in institution building within society may invite emotive responses to the perceived political legitimacy of the former. Nevertheless, the emphasis which he puts on the role of paramilitary forces in enhanced regime consolidation not only justifies the publication of his revised essay but also serves to remind us of the fragile nature of political institutions within the developing countries. What we now need is further research which will, on the one hand, debate theoretical and methodological core issues and, on the other, provide documentary materials to test the validity of the hypotheses. What we do not need is ill-informed and emotional reaction which ignores the increased political persistence of military regimes in the developing nations.

Academic Power in Italy: Bureaucracy and Oligarchy in a National University System. By Burton R. Clark. Chicago: University of Chicago Press, 1977. Pp. xi+205. \$15.00.

Roberta Garner
DePaul University

Burton Clark has written an extraordinarily interesting case study of the Italian university system, focusing on national and micro structures and especially on the exercise of power. *Academic Power in Italy* will be valuable to anyone with an interest in complex organizations, the sociology of education, public administration and national policymaking, comparative analysis, and theoretical issues concerning power and authority.

Clark's convincing main point is that the Italian university system retains strong guild features, with power in the hands of an oligarchy of chair holders. The system is thus relatively insulated against the state bureaucracy and market forces.

The university system shares these features with a number of other Italian institutions. Benito Mussolini once remarked that "governing Italians is not difficult—only pointless [*inutile*]." Americans have misread this quip to fit their stereotype of Italy as a chaotic country; in truth the remark is a good deal more perceptive. It draws attention to the fact that in many respects Italy only appears to be a modern nation-state with a centralized legal-rational state bureaucracy; in reality it is a self-regulating society in which the modern state is of only minor importance compared with a host of institutions of largely precapitalist vintage. The functioning of the society is based on primordial ties, guild structures and corporatism, clientism, custom and mechanical solidarity, and a good deal of common sense and human warmth. The "pointless" modern state has been superimposed on these structures and habits, but in large part is dependent on them and limited by them. Antonio Gramsci remarked on the reliance of the modern capitalist state on traditional intellectuals—ideologues and functionaries associated with leftover precapitalist institutions who have a vital part in sustaining the present state and social order.

Clark's book shows us how this general pattern of Italian social structure plays itself out in the university system. Underlying what appears to be a rather centralized, state-dominated, and bureaucratic structure for the national university system is the reality of a guild structure run by the chair-holding professors, who exercise their power through networks and pyramids of personal relationships. Within this structure, students and untenured faculty are dependent, university administrators are very weak, and administrators in the national bureaucracy have only limited powers. The structure has medieval origins but survived until the 19th century and thereafter was sustained with some modifications by Italy's succession of modern state forms: the liberal state, the Fascist state, and the postwar republic.

The methodology on which these conclusions are based is evidently a blend of observation, use of government documents and historical materials, and a generous portion of Weberian *verstehen*. Those who believe only in numbers may be dissatisfied, but I found that the conclusions corresponded closely to my own observations and to Italians' understanding of their system.

Clark's largely implicit evaluation of the system is less convincing than his description of it. Undoubtedly the Italian system has had severe problems, particularly in adjusting to an influx of students in the late 1960s and the 1970s. Its productivity in the natural sciences has perhaps been spotty, although the evidence is not unequivocal on this point; underfunded or poorly organized research may be caused by Italy's general scarcity of resources far more than by structural weaknesses of the university system. In any case, some reforms are clearly in order; the 1973 reform of promoting many untenured faculty seems to be a step in the right direction, and a stronger development of departments may be useful.

Yet Clark's evaluation is perhaps too hard on the Italian system and too uncritical toward the American one. In the final analysis, is a system of authoritarian relationships embedded in a bureaucratized system of largely fixed funding and national standards (the Italian pattern) really less effective than a system of commodity relationships in an academic labor market embedded within a system of a competitive search for funds and pressures from private industry (the American pattern)? I remain unconvinced that the American system of market forces, competition, widely varying institutional quality, influence of lay trustees, and relentless pressures toward data accumulation and knowledge specialization is any more conducive to real intellectual endeavor than the Italian system. And I am not sure that the young American specialist as "a prized [and increasingly, not-so-prized] commodity" (p. 122), trapped in transitory and highly instrumentalized relationships, is any better off than the young Italian scholar trapped in the patriarchal forms of the guild structure. To even begin to answer such questions, we need a careful comparative evaluation, with more attention to the "outputs" of intellectual productivity as well as to general educational level; above all, we need a clearer identification of goals for educational systems.

In fairness to Clark, he does not explicitly prefer the American system to the Italian one (although the preference seems to me implicit in his chapter on reform), and in the last chapter he returns to a relatively even-handed analysis of the admixture of guild, bureaucratic, and professional forms in several national university systems. Certainly Clark is sensitive to the fact that all modern university systems face extensive problems of student expectations and preparation and of resource allocation; there are no simple choices to be made among bureaucracy, technocracy, guild oligarchy, market mechanisms, and other possible models for determining university structures. He draws our attention to the difficult questions: At which level should power be concentrated? What criteria should be used for judging students, faculty, and the performance of the whole

system? How should national resources be allocated? Should a system have a uniform set of goals or a differentiated one? What are the costs of reducing professorial power and increasing that of national- or university-level administrators? In what ways—if any—should university elites exercise power in the national political system?

Although some of the value assumptions could be disputed, the book makes a contribution to our understanding of national university systems and tackles the issue of national educational policy. Its sensitivity to historical issues and its successful situating of a complex organization within a national social structure are particularly welcome. Its lucidity, readable style, clear organization, and attractive format will make it appeal to educated laymen and undergraduates as well as to specialists in the field.

Churches and Church Goers: Patterns of Church Growth in the British Isles since 1700. By Robert Currie, Alan Gilbert, and Lee Horsley. Oxford and New York: Clarendon Press, 1977. Pp. xi+224. \$29.95.

N. J. Demerath III

University of Massachusetts, Amherst

Churches and Church Goers is a work to be envied by every scavenger for truth in the scrapyard of church statistics. Using more reliable data over a far longer haul than Americans are used to, the authors have produced a triumph of imagination as well as rigor. Fully half the book is a data repository for those who would take the analyses further. But the authors set high standards for scholars in their wake. It is hard to overestimate the amount of care and effort represented by the statistics themselves (e.g., some tables have notes which alone extend for several pages). But to cite the work of the legs is not to slight the work of the minds.

This is not a statistical history of British religion. It is something both more specific and more generalizable. Currie, Gilbert, and Horsley examine the range of variables affecting church membership—and, implicitly, membership in voluntary associations of all sorts. The book's principal thesis is that church policies and recruitment strategies can make a difference in the short run but are eclipsed by "exogenous" influences in the longer run. The authors offer a variety of theories of church growth from which I have excerpted a three-stage model as a summarizing framework.

First, any church growth depends upon an "external constituency." The size of this constituency depends upon a range of factors at work on the population at large. The authors provide searching discussions of secularization, industrialization, urbanization, trade cycles, political crises, and war—all exogenous phenomena beyond the reach of church policy. However, churches can engage in "indirect recruitment" to prime the pump of potential membership. Here a historical analysis of Methodist Sunday schools provides a case in point.

Second, given an external constituency, the trick is to render it internal; that is, to create a cadre of "non-member adherents." Here church policy can be more instrumental—for example, by maximizing parish proximity to potential members. But the important variable of cultural and socioeconomic "congruity" between members and potential members is often more difficult for churches to manipulate. Even successful past recruitment can produce a membership that is incongruous with otherwise likely future members.

Third, after a church has found or generated an internal constituency, direct recruitment can be effective. A church may attract members on the basis of its primary, secondary, or tertiary functions (i.e., for supernatural means to ultimate ends; for ritual as an end in its own right; and for organization as a political or economic instrumentality). Potential members must perceive some "utility" in the church on one or more of these terms. But it makes considerable difference whether recruitment is "autogenous" (from within the families of existing members) or "allogeneous" (from among the families of nonmembers). The distinction underlies a fundamental paradox: "Higher growth cannot be achieved by simply recruiting church members' children, since the stock of such children is limited; and therefore children of non-members must also be recruited if churches are to grow more rapidly. But non-members' children are significantly less likely than members' children to retain membership; and therefore *increased recruitment must involve increased losses*, losses which will occur within the time-span of the average growth cycle" (pp. 43–44). The authors emphasize membership processes and cycles, adroitly using their statistics to illustrate two somewhat redundant phase models: first, progressive, marginal, recessive, and residual; and second, depression, activation, revival, deactivation, and declension. They summarize the basic dialectic with characteristic pith and insight:

First, each new member gained is an adherent lost; and, since it seems to be easier to make members than to gain adherents, there is a time lag between making an adherent into a member and replacing him with a new adherent. Secondly, any increase in recruitment means acquisition of less committed members; and any decrease in recruitment generally means acquisition of more committed members. Thus a rise in the recruitment rates reduces the possibility of further rises, while also raising the loss rate; and the fall in the recruitment rates reduces the possibility of further falls, while lowering the loss rate. At the end of a rise in recruitment, the pool of adherence is depleted; at the end of a fall in recruitment, the pool is replenished. [P. 120]

As these quotations suggest, the authors have a keen sense of the ironic. But perhaps the most ironic aspect of the book is its subtitle and principal justification: to emphasize patterns of church "growth" and to argue that "perhaps *the* great missing problem in the sociology of religion is the problem of growth" (p. 4) is at best peculiar in the British context. Not surprisingly, this is largely an essay on church decline, at least from the latter

half of the 19th century. More surprisingly, the authors do not aggregate their statistics to test such sweeping statements. They are more concerned with denominations, churches, and institutionalized and noninstitutionalized sects than with some hypothetical construct of "British religion." The authors are also fully aware that their church records of membership must be distinguished from other measures of religious participation (though their defense here is a bit strident), and they do supplement them with demographic data and time series concerning such matters as civil marriages and audiences for BBC religion broadcasts.

Throughout, the authors seem content to illustrate their insights rather than test them. They make no use of sophisticated statistical techniques. Even simple correlations appear only once (to show that membership series for different denominations are parallel through time), and these coefficients are extended only to a single decimal—a perversely refreshing change from the pursuit of spurious precision with three and four decimals.

There is a becoming modesty about this volume. But it is very *useful* scholarship indeed, in the literal and noble sense of that term.

Marginal Workers, Marginal Jobs: The Underutilization of American Workers. By Teresa A. Sullivan. Austin: University of Texas Press, 1978. Pp. xv+229. \$17.95.

Charles Hirschman
Duke University

At the end of the 1960s, economic policymakers thought they had the unemployment problem licked. Management of economic demand through fine tuning of government fiscal machinery, they believed, would all but eliminate the cyclical ups and downs of the economy that periodically threw millions of workers into the unemployed ranks. As we now enter our third recession in the past 10 years, the possibility of a full-employment economy seems as distant as ever. The intense public attention and debate over unemployment often obscure comparable problems among the regularly employed during recession and prosperity. Many full-time workers receive wages below the poverty line, and others have jobs that demand little use of their skills and training. There are many jobs that could easily be labeled potentially hazardous to the worker's physical and mental health.

Public policy and even academic study on the quality of employment are hindered by a lack of consensus over the attributes and measurement of "underemployment" or "labor underutilization." In *Marginal Workers, Marginal Jobs*, Teresa Sullivan presents a conceptual and measurement framework to address these issues and then empirically analyzes the extent and patterns of labor underutilization in the United States. Her work is the most comprehensive application of the "labor utilization framework" originally developed by Philip Hauser to study underemployment in de-

veloping countries. The macroeconomic or structural causes of labor underutilization are not addressed in this work; rather the focus is on the incidence of underutilization in various subpopulations classified by race, sex, age, and other sociodemographic characteristics.

Sullivan presents a conceptual rationale and measurement techniques to identify four components of labor underutilization: unemployment, underutilization by hours of work, underutilization by level of earnings, and the mismatch of educational attainment and occupational position. Thus a worker can be classified as underutilized if he or she is unemployed, works part time and desires more hours of work, receives wages below the poverty level (adjusted to the family status of the worker), or has an education considerably above (one standard deviation) the average for the occupation. Using 1960 and 1970 census data and other national survey data, the author finds that marginal workers (identified as the young and old, minorities, women, and others in certain sectors of the economy) are most likely to be underutilized. These differentials are interpreted as the result of discrimination by employers who prefer white, male, and middle-aged workers. Other interpretations are developed for the higher incidence of underutilization among the physically handicapped and workers in marginal occupations and industries. As with any pioneering work that claims to formulate a paradigm for a muddled field, there are bound to be sharp disagreements over the conceptual approach and measurement strategies outlined in this book. Sullivan carefully anticipates many potential objections but asserts that the labor utilization framework is better than all the alternatives. However, I remain skeptical that Sullivan and Hauser's proposed approach will be widely accepted as the model for future studies of labor underutilization.

The most basic flaw, in my opinion, is the idea that the sum of various types of underemployment can be a meaningful index of labor underutilization. The causes of poverty-level earnings and of the mismatch of skills and occupational roles are quite different, as are their possible policy remedies. To be useful in either the policy or academic realms, social indicators must have a straightforward interpretation, and the summary index of labor underutilization does not. Even though most (but not all) analyses in the book show all four components of underutilization, the hierarchical principle of classification (the categories are mutually exclusive, with the most underutilized status taking precedence) shifts an unknown proportion of each underutilized status into other categories. I would prefer keeping each type of underemployment a separate dependent variable and not merely a separate category of a common underutilization index.

Lacking any theory of how employment institutions generate jobs of varying qualities and rewards, the selection of "types of underemployment" is a rather arbitrary task with few precedents for guidance and only conventional labor force data available for reanalysis. Having spent many hours in the same murky terrain, I understand the problems faced by the author. Yet on the basis of her own criteria, I question the selection of some of the labor underutilization categories and the nonselection of others in the

present work. Most serious is the omission of discouraged workers from the unemployment category. Sullivan argues that to include them would impair comparability with the standard labor force measure, but this could easily be handled by presenting two definitions of the labor force (standard and adjusted). She presents the basic types of underemployment with reference to the goals of individuals ("private underutilization"), which may be conceptually independent of the loss to the economy or society at large ("social underutilization"). According to this logic, one must select underutilization measures which represent substandard intrinsic or extrinsic rewards. These could be defined individually by the subjective standards of each worker or by reference to "objective" standards which represent common expectations of the minimal rewards that employment should provide. Sullivan opts for the "objective" path of measurement, but one of the categories of underutilization (insufficient hours of work) is operationally measured by the subjective preferences of each worker. Moreover, insufficient hours of work do not seem to be a "real" condition of substandard employment. Insufficient hours of employment are probably considered undesirable by most workers only because they lead to low earnings. The mismatch of education with occupation appears to be a case of "social" rather than individual underutilization. On the individual level, an alternative indicator is required—one that taps the intrinsic rewards that employment provides to workers. I do not have an optimal resolution to this problem, but subjective judgments of "quality" of employment can hardly be avoided.

After detailed discussion of the theoretical and measurement problems, Sullivan goes on to test a series of hypotheses regarding the association of individual-level characteristics and labor underutilization. In the standard tradition of social demographic analysis, she relies primarily on cross-tabular methods which are augmented with Goodman log linear models. I found the style and organization of the analysis to be rather awkward and inefficient. Age, race, and sex are considered as independent variables in separate bivariate analyses before being brought into a comprehensive multivariate analysis. This is inefficient because the gross effects of age or race on any labor force variable, without simultaneous controls for sex, provide a nebulous interpretation. The multivariate-analysis chapter confuses the search for statistical interactions with the joint "additive" effects of the independent variables. Because natural interval-scale variables (low earnings, mismatch) are dichotomized, much of the potential power of the analysis is lost.

As much as I question some of the concepts, measurement strategies, and analysis in this book, it is not a work to be ignored. It raises issues which have long been neglected in labor economics and the sociology of work. It presents a strong case for a particular approach to the measurement and analysis of underemployment. For those of us who find it an unsatisfactory direction, it is necessary to propose a better alternative. That will not be an easy task.

Social Influence and Social Change. By Serge Moscovici. Translated by Carol Sherrard and Greta Heinz. New York: Academic Press, 1976. Pp. ix+239. \$15.25.

Vernon L. Allen

University of Wisconsin—Madison

Social Influence and Social Change is the tenth book in the series of European Monographs in Social Psychology, published under the auspices of the European Association of Social Psychology. The author, Serge Moscovici, is a professor at the Sorbonne and one of the leading European contributors to social psychology.

Historically, social influence has been one of the most important topics in social psychology. Most research and theory have treated the topic of social influence as practically synonymous with the study of conformity or compliance to pressure from a group (or other influence source). It is this traditional conceptualization of social influence that Moscovici takes to task in his book. Labeling current thinking about social influence as the "functionalist" approach, he attempts to lay bare the implicit assumptions underlying our thinking in this area by stating six propositions that are then closely scrutinized and subjected to severe criticism. According to Moscovici, social control is the crux of the functionalist approach to social influence; that is, the basic problem is identified as the adjustment of the individual to the social system. In this model power is the source of influence, and the dependence and uncertainty of the individual account for the effectiveness of attempts to influence. Moreover, research and theory treat social influence as flowing only from the source to the target—a unidirectional relationship.

Placed in direct opposition to the functionalist view of social influence is the "genetic" model espoused by Moscovici. The basic assumption of this model is that social influence is reciprocal—that each person in a group is both a potential source and a potential recipient of influence. The central role of social change and innovation is emphasized instead of conformity and social control. The production and resolution of conflict are viewed as the means by which social influence is exercised. As modes of influence, innovation and normalization (compromise) as well as conformity are included. From these propositions it follows that a minority or subgroup—though lacking in power—can, nevertheless, successfully influence the majority.

Moscovici supports the new model with argument and experimental data. Turning the usual problem of social influence on its head, he designed several experiments to examine the minority's influence on the majority. The author contends that, by using the technique of behavioral style, a subgroup can successfully influence a majority. Three types of behavioral styles are discussed—investment, autonomy, and consistency—but only consis-

tency is subjected to experimental analysis. Consistent behavior presumably creates conflict and doubt in members of the majority, thereby leading them to reassess their own position.

Using small group studies in the laboratory, Moscovici and colleagues have shown that, by consistently advocating a position, the minority can produce a significant change in the responses of the majority. Thus, intransigence can be an effective influence strategy. One of the most intriguing findings reported in the book is that the minority seems to exert an even stronger effect on the majority's private perceptions and beliefs than on their public responses. The experiments reported in the book are, however, susceptible to criticism. Results are sometimes not as clear-cut as one would wish, and alternative explanations are available for other findings. The procedure used in the studies does not allow for discussion between the minority and the majority, and the stimuli that are used by subjects in making their judgments are usually quite ambiguous.

Throughout the book the author uses results of experimental studies as a starting point for discussing current political and social events involving minority-majority relations. These observations reveal the author's sympathetic understanding of the difficulties encountered by a minority attempting to produce a change in society. Several insightful analyses are provided in the course of discussing the complex psychological issues involved in the uneasy relationship between a minority and a majority. However, in spite of the exclusive emphasis on behavioral style (consistency) as an influence strategy, it is still an open question whether behavioral style is as important in the real world as Moscovici claims. Other tactics may far outweigh mere behavioral consistency as a successful influence technique in the hands of a minority.

Social psychologists who are seriously interested in problems of social influence will be stimulated by this important book. The author has performed a valuable service by challenging our traditional conceptualizations of social influence. Although he sometimes exaggerates the deficiencies of previous theory and research and sometimes selectively cites literature to illustrate a point strongly, the general contours of the critique are undoubtedly valid. Moscovici has taken the role of a lone critic against the establishment's position, and he repeatedly asserts that it is indeed possible for a minority to influence the majority. Thus, the author himself uses consistency as a behavioral style throughout the book. Although his may be a lone voice at present, if the theory is correct we should all be inclined to accept it after having read this book.

Policing the Crisis: Mugging, the State, and Law and Order. By Stuart Hall, Chas Critcher, Tony Jefferson, John Clarke, and Brian Roberts. New York: Holmes & Meier, 1978. Pp. xii+425. \$28.75 (cloth); \$14.50 (paper).

Austin T. Turk
University of Toronto

This recent contribution to British Marxist criminology examines "mugging" as a product and indicator of the developing crisis of British and world capitalism. *Policing the Crisis* begins with an account of how and why there was a "moral panic" in 1972-73 over the factually unsupported appearance and increase of the "new" crime of mugging. The term was imported from the United States as a complete symbolic package—street crime, black threat, poverty, violence, breakdown of law and order, all adding up to "general social crisis and rising crime" (p. 23)—which fitted readily into the growing conservative mood and movement against "permissiveness." In an analysis of the social production of crime news, Stuart Hall et al. adduce supporting evidence from police and court records, statements by officials, media reports and editorials, and letters to the newspapers.

Liberal attempts to counter or soften the punitiveness of the law-and-order forces are found to have been ineffectual because liberal analyses inevitably fell short of an adequate, structural explanation couched in terms historically specific enough to challenge the "commonsense" traditionalisms (or myths) of the English middle and working classes. Instead of concrete analyses offering true explanations, "public images" were invoked that merely suggested fragmented, descriptive connections among various "social problems"—thus accomplishing only "rhetorical closure" (p. 118).

A chapter is devoted to analyzing the bases in English class cultures of the prevailing explanations and ideologies of crime. The concluding theme is that the mugger was (and is) the perfect scapegoat for all classes, a "Folk Devil" reflecting "the fears and anxieties of those who first imagined, and then actually discovered him: young, black . . . threatening the traditional peace of the streets . . . embodying in his every action and person, feelings and values that were the opposite of those decencies and restraints which make England what she is" (pp. 161-62).

Why did the British go looking for such a Folk Devil? The authors argue that the reason lies in the limited, unsatisfactory answers offered within the ideological constraints of British capitalism which leave people floundering as the crisis grows—the crisis of a weak postimperial economy; of the deteriorating capacity of the established parties to mobilize and control political activity; of the transition to an interventionist, "late capitalist" state; and of the decline of political legitimacy, hegemony, and authority in general (pp. 317-20). "Iron times" are coming, because "Britain in the 1970s is a country for whose crisis there are no viable capitalist solutions left, and where, as yet, there is no political base for an alternative socialist

strategy. It is a nation locked in a deadly stalemate: a state of unstoppable capitalist decline" (p. 309).

There is here a rich store of assertions, concepts, and accounts to stimulate scientific research and theory. Despite being tendentious, verbose, and more magazine-intellectual than scientific in style and content, this is an important book. Social scientists are likely to be exasperated by the effort required to plow through it all—including long discourses on Western history and general Marxian thought—to find the useful bits, but they will find many. As is to be expected, the critique of capitalism does not recognize that essentially the same points can be made in analyzing the current crises of the Soviet, Chinese, Vietnamese, Cuban, and other state socialist polities. As for other theoretical perspectives, the writers do note the "control-culture" concept of the labeling analysts but see it as too ahistorical to be of any but general descriptive use. More developed non-Marxist structural theories of social conflict are not mentioned, presumably because they are viewed as no more specific than the "control-culture" notion or, at best, as variants of power pluralism that miss the realities of domination and change. Whatever one may think of the one-sidedness of Marxian analyses, to deal with them requires demonstrations in empirical research that scientific analyses of the structural dynamics of social conflict and power can be more predictively powerful than are the treatises of Marxian intellectuals.

Swindling and Selling. By Arthur Allen Leff. New York: Free Press, 1976. Pp. vii+194. \$10.95.

Peter K. Manning
Michigan State University

Swindling and Selling is written by a law professor who strives to bring about "a marriage between the approaches and vocabularies of two different disciplines, economics and sociology" (p. 179). Economics, Arthur Leff claims, tends to use "snapshots" of equilibrated states instead of attempting to explicate the dynamics of transactions. And too often sociology has captured the dynamics of human choice, "the state of which at any particular moment the economists seemed better at describing" (p. 180). The two approaches are seen as complementary transformations of each other.

Leff has an engaging style, characterized by heroic animated villains such as "Marvin Sonnenlieb," two- and three-part "playlets," "public spectacles" such as "Godcons" and "ancient estates," and a keen analytic eye for identifying some logical similarities between swindling and selling. Focusing on the "heartland of commerce at which supply and demand meet" (p. 6), he argues that all competition has a cooperative dimension so that, while each competitor tries to maximize his utility at the expense of others, it is only together that they can increase the total utility of their joint system:

"It is over their respective shares of this newly created potential value that the parties really contend" (p. 14). The assumptions of joint interest, joint dependence, and joint distrust (self-interest) energize the examples provided of swindlers and sellers. The book is roughly divided into four sections, the first dealing with the general properties of swindling and selling, the second and third explicating swindling and selling micro-encounters through scenarios, and the last analyzing "cooling out." The sociology of Erving Goffman is called upon to sanction the dynamic analysis of the episodes.

Economically, the person in these playlets is a rational, information-seeking, and information-utilizing minimaxing entity, while dramaturgically, the person is a front-presenting, expressively sparkling poseur who values appearances. The guiding assumptions are that the value of what is at issue is known, that the agreement to bargain is made, and that what is offered is valuable to each and is to the advantage of both to transfer. (When a third person enters the system, the differentiation of the market produces other choices, also seen within an interacting utility matrix.)

Since Leff makes certain assumptions about "man" and about the nature of games or micro-interactions, in effect he is analyzing closely only a special subset where the rules he poses operate. Unfortunately, other rules also operate in these domains and are neither omitted by the economic model nor defined as outside the systems described. The basic proposition of the book, it appears, is that, since people are *Homo economicus*, if they are swindled it is because ego is made to believe in the first instance that alter has something ego wants and is willing to pay for. Leff fleshes out this proposition, showing how the social reality of elements of transactions are communicated through such mechanisms as altercasting, casting, and face-work.

There are three unresolved problems in these sorts of analyses, and they mark the limits of the generality of Leff's interpretation. Some aspects of the argument seem to be stuck somewhere, like a deer in the body of a python. The first difficulty creeps in cautiously in a phrase, "in the interaction situations explored here, every man is in fact assumed to view every other man the way economists view all men [as] an economic man" (p. 11). This frames the interactions at issue as those controlled by the rules constituting what Goffman calls "tightly organized games." These are games based on information, utility, and clearly understood payoffs. These sorts of games are known to players, the fatefulness is recognized, and concrete courses of action are monitored. In addition, however, such an argument requires that one assume that decisions are made by orienting oneself to the other, giving weight to the situation as the other seems to see it, including his giving weight to one's own perspective (Erving Goffman, *Strategic Interaction* [Philadelphia: University of Pennsylvania Press, 1969], pp. 100-101). The episodes used as representative of "swindling and selling" are analyzed in formal terms (e.g., by rules) rather than in dynamic terms, in spite of Leff's quest for a dynamic explanation. The rules invoked are something like the rules used to formulate the choice patterns of undergraduate students who are manipulated in the pseudorational hypothetical world of

experimental social psychologists. How are such interactions themselves framed, cast, marked off, and distinguished from loose games, fabricated cons, rekeyed fabrications, and practical jokes? These interactions resemble "swindling" and "selling," but one never knows how that form of life constituted here as swindling/selling could be contextually recognized.

Another difficulty is that, even within the frame of a tight game, the element of *trust* is not adequately explored by Leff. The basic mechanisms in gaming are not, as he asserts, information, desire for gain, and an expectation that interactions will produce emergently higher utilities for participants. The basic mechanism is trust. For example, middle-level drug dealers generally do not deal to people who are of different ethnic, class, and territorial origin, and race is especially powerful as a filter. And if they do deal to such people, these exceptions are identified as potential snitches (police informants). Similarly, apparently tight information games such as double agency and narcotics agents who are handling informants are based on a useful oxymoron, "suspicious trust." These interactions are swindling/selling microcosms, at least originally. Trust as a socially problematic mechanism, since it is always present in all games, suggests that stripped situations such as those described by Leff are sufficiently spare to be sparse.

Leff does allude to the centrality of belief in the existence of an object and how it is both created and cooperatively sustained by swindlers and sellers. The belief is based on information supplied by the con man to the mark and, once believed by the mark, becomes part of the interactional system. Information games are rather special cases of those micro-interactions in which all the requirements of tight games are met in addition to the suppression of all expressive particulars, the little indications that there is a person who acts with unique style behind the role—a person with nonrational aims and values who may act on ignorance and/or error. Perhaps the omission of the last two has been adequately criticized by critics of utilitarianism such as Talcott Parsons. But since Leff claims to be integrating Goffman and economic analysis, an example of expressive particulars deserves brief mention. A shifted posture, a furtive glance, a smile at the wrong time, as well as other nonverbal leakage may provide the clue that such "disattended" information is the most important information of all. This is certainly true in all sorts of swindles, such as drug policing and the "working of informants." It is not at all clear that "externalities" are external to the microeconomic analysis of socially grounded transactions.

Leff argues for a dynamic transactional analysis while excluding the socially problematic features of framing of interactions, trust, and expressive particulars. The legal relevance of such an analysis appears to be that regulating such transactions is problematic not only because, though legally distinct, they are structurally similar in their consensual trust basis, but also because the law has sought to "make subjective mental states legally determinate" (p. 183). Although the work is intriguing and intriguingly presented and argued, it fails to delimit fully the scope of the

argument and, as a result, blurs certain important distinctions. Nevertheless, it is a challenging and thoughtful treatise.

Odd Jobs: The World of Deviant Work: Confidence Men, Fences, Fortune Tellers, Strippers, Medical Quacks, Prostitutes, Female Impersonators, Bookmakers, Racketeers, Safecrackers. By Gale Miller. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1978. Pp. xii+260. \$12.95 (cloth); \$4.95 (paper).

Joseph Harry

Northern Illinois University

The subtitle of *Odd Jobs* indicates the topics dealt with in the substantive chapters. Gale Miller attempts an analysis of the organization of the social roles surrounding each of these forms of work. He gives particular attention to the work-role problems encountered by these deviant workers and approaches them largely from a sociology of occupations perspective.

While all the chapters are good, the best are those dealing with theft, false pretense, and the rackets. Comparing older and more recent studies of professional theft and con games, Miller argues persuasively that these activities no longer fit the model of professional theft offered during the 1930s by E. H. Sutherland. These activities have undergone a significant measure of deprofessionalization. Theft, such as burglary, has been invaded by juvenile amateurs, while con games have become more like white-collar crime, dealing in larger amounts of money, having a fairly stable place of business, and lacking a community of similar confidence men who share common norms of loyalty. Also, professional theft and confidence operations now differ from those of the 1930s since fixes are much harder to come by.

Miller gives considerable attention to the changing nature of deviant forms of social organization in response to changes in the broader society. For example, the prostitutes of the glory days of the American bordello were replaced by contemporary streetwalkers and call girls who in turn are experiencing competition from massage parlors. Such constant social change gives rise to economic opportunities for innovative deviants to develop new forms of deviant organization, just as Jonathan Wild, the founding father of fencing, responded to changes in the early 18th century. As a result of such innovations, deviant activities can become more bureaucratic (e.g., organized crime) or less professional (e.g., burglary).

The effects of change on the social organization of female impersonation are also considered by Miller. Basing his analysis almost totally on Esther Newton's fine ethnography of female impersonation, *Mother Camp* (Chicago: University of Chicago Press, 1972), he argues that changes away from stereotyped effeminate behavior among gay men have made impersonators less appealing to gay audiences. Hence, "the future of female

impersonation is not very bright" (p. 191). It would thus appear, I suggest, that impersonators of the future may play largely to straight audiences rather than to gay ones, or to gays in smaller cities.

The last substantive chapter in the book deals with the role of organization in two deviant settings—the after-hours bar catering to various underworld types and the gay bar—rather than with a principal deviant role such as that of the prostitute. The participants in these settings also differ from those discussed in previous chapters in that they are "deviant groups within the society that are primarily leisure groups" (p. 220). Hence, though good substantively, the chapter somewhat strains the author's approach—deviance as work. A minor fault in the discussion of the gay bar is that Miller does not seem to recognize the diversity of such bars. Basing his discussion on the somewhat dated works of Achilles and Hooker, he concentrates on the bars of young gays and ignores gay bars for Latino gays, black gays, and older gays. Borrowing from Hooker, he also somewhat exaggerates the degree of turnover of bars in a city. For example, Kitty's, a bar catering to well-to-do, older (40–60) gay men, has existed in Chicago for approximately 35 years.

Miller's book, while presenting no new data, is an intelligent and informed discussion of a number of issues in the area of deviance. For example, in his chapter on organized crime he cogently addresses the issue of the extent to which such crime is nationally organized, centralized, and bureaucratic or is better viewed as a community of deviant businessmen. His style of writing is clear. The book would be highly useful in a deviance or perhaps a criminology course. I plan to use it the next time I teach sociology of deviance.

On the Take: From Petty Crooks to Presidents. By William J. Chambliss. Bloomington: Indiana University Press, 1978. Pp. xii+269. \$10.95.

John R. Hepburn
Pennsylvania State University

Crime, William Chambliss asserts, is the product of the political economy of American cities. The current and pervasive view of organized crime, largely a reflection of grand jury records, files of attorneys general, and investigatory commissions, is so distorted as to detract our attention from the symbiotic relation between organized criminal activities and bureaucracy. The people who organize and profit from the criminal activities in Seattle have legitimate careers in business, politics, and law enforcement and are neither of Italian descent nor members of a criminal culture. Vice and corruption are possible only because bureaucratic organizations are characterized by discretionary decision making and rule enforcement, generally directed to the minimization of organizational strain and the maximization of organizational reward.

If these words sound familiar, it is because they were the substance of Chambliss's "Vice, Corruption, Bureaucracy and Power" (*Wisconsin Law Review* 1971 [1971]: 1150-73). That analysis of organized criminal activity in Seattle is the basis for *On the Take*. As in his earlier work, Chambliss relies heavily on informant interviews and personal observations recorded from 1962 to 1969. In some respects the monograph is an ethnographic elaboration of the vice and corruption activities in Seattle during that time. But there are some important differences.

The criminal activities are no longer seen as headed by a "syndicate" or "cabal" but as the domain of a diffuse "network," presented by Chambliss as a stratified organization comprised of financiers at the top of the structure who support the middle-management organizers, a collection of persons representing business, politics, and law enforcement, who supervise the lowest level, the racketeers. The elaboration of this network and its characterization as an uneasy alliance of persons bound by a shared interest in ill-gained profits are a noteworthy addition. The network insures cooperation by using part of the profits for campaign contributions and payoffs "to grease the wheels of industry"; such collusion is so pervasive and far-reaching that it involves state and national political leaders, including Presidents Johnson and Nixon.

As in the earlier work, Chambliss uses observation, interviews, and informants to present a thorough analysis of the racketeers, the lowest level of the network. The purported involvement of individuals at the top of the hierarchy, however, lacks the depth of documentation expected of a good journalist. Chambliss asserts, for example, that "politicians who were *deeply involved in the network* met regularly at their 'businessmen's club' with members of the city council, the county board of supervisors, and several key businessmen who were *profiting from the rackets*" (p. 64; my italics), an accusation based on innuendo, allegation, and life-styles. Difficulty in obtaining valid and systematic data on the hierarchy of the network is not uncommon in the study of the powerful. Although the author was referring to attempts by local journalists to investigate and reveal the network's penetration into legitimate businesses and local politics when he observed that "attempts to expose more than the most visible, least important aspects of network operations had always failed" (p. 114), some may wish to argue that Chambliss has failed also. The lack of documentation gives rise to a sense that the author is building an organization of payoffs, collusion, and bribery out of a fragile, tenuous, and not entirely objective interpretation of events. In the light of Chambliss's argument that data from police, files of attorneys general, and grand jury records have presented a distorted picture of organized crime, it is ironic that he relies on a grand jury indictment of 54 politicians and law enforcement officials in 1971 as the major source of data demonstrating bribery and collusion.

The most dramatic shift from the earlier article is the author's attempt to expand the basis for the emergence and survival of organized crime from the contradictions of bureaucracy to the contradictions of capitalism. Un-

fortunately, Chambliss is unable to demonstrate the linkage convincingly. True, the rhetoric is there: capitalism, based on private ownership of property accumulated by selling labor, products, or services, results in an effort to meet the high demand for profitable but illegal goods and services; since this illegal behavior needs the cooperation of law enforcement officials to continue to operate, the profits are used to purchase the necessary protection; since the cost of political campaigns is great, the profits are invested to corrupt public officials; since illegal criminal activities are a business, it is efficient and profitable to organize those activities as one would any business in a capitalist state. However, there is little in this argument that is novel. That persons coordinate their behavior for the purpose of criminal activities is a testimony to the efficiency of formal organization and is not inherent in capitalism. That the collusion and payoffs touch the level of state and national figures is less an indictment of capitalism than an indictment of the high cost of running for office. Thus, while capitalism may indeed give rise to the profit motive, capitalism and its contradictions cannot account for the integrated, organized criminal network which reaches even the highest political levels.

Recent attention has been directed to white-collar and corporate crime. Chambliss points to yet another example of criminal activity by the powerful and respectable. In this particular case, however, the type of crime is more traditional, less sophisticated, and more likely to be viewed erroneously as limited to street urchins and petty entrepreneurs. The value of the monograph rests on its ability to raise the issues of structural, even capitalistic, inducements and strains toward organized criminality and the routine involvement in that criminality by every aspect of our society.

Morals and Markets: The Development of Life Insurance in the United States. By Viviana A. Rotman Zelizer. New York: Columbia University Press, 1979. Pp. xiv+208. \$15.00.

Carol A. Heimer
University of Arizona

One of the great unsolved mysteries in the history of insurance is the sudden success of life insurance (in the 1840s in the United States) after decades of failure. The usual answers have a distinctly economic flavor and include the general commercial and financial development of the country, the growth in the purchasing power of the populace, the dependence of an increasingly urban population on money incomes, changes in life insurance policies that made them available in smaller sizes and protected the interests of policyholders, a switch from other marketing techniques to the agency system, and so on. Without completely discounting all these arguments, Zelizer contends that they at least need to be supplemented with a consideration of noneconomic factors. In particular, she

argues in *Morals and Markets* that the sudden popularity of life insurance was due to a change in the public view of the relation between life insurance and death and also to the increasing acceptability of certain forms of speculation. In other words, life insurance caught on only when insurers mastered the delicate problems of fixing a price on death and learning to sell pessimistic futures.

Zelizer sets up her problem in the first two chapters. The third chapter examines cross-national variations and variations between lines of insurance. Life insurance caught on more slowly in France than in the United States and more slowly in the United States than in England. This, Zelizer argues, was because of cultural variations in views on the appropriateness of contracts dealing with the economic consequences of death. Similarly, life insurance caught on more slowly than either marine or fire insurance, against which there were no moral objections.

The next two chapters discuss the particular noneconomic factors that Zelizer believes inhibited the diffusion of life insurance. Using as evidence discussions from insurance periodicals and training manuals, the popular press, and sermons, Zelizer makes her case for the impact of values on the adoption of life insurance. Opposed by one set of values, life insurers associated their product with an alternative set. If on the one hand life insurance was unacceptable because of magical beliefs about death and because it involved financial evaluation of human life, it was on the other hand much more acceptable as part of an emerging active orientation toward death and as a continuation of the traditional symbolic association between money and death. Similarly, when life insurance was attacked because it challenged the role of Providence, undermined industriousness and self-reliance, and generally disrupted the social and religious order, its supporters answered that man's duty was to provide for himself as well as he could and that life insurance should be compared with efficient risk management in business rather than with gambling.

The final two chapters discuss the problem of structural ambivalence within the life insurance industry. In a chapter on the marketing of life insurance, Zelizer records the changes in the ideology of life insurers (from an image of insurance as a beneficent institution to unabashed commercialism) and the corresponding changes in the characterization of the life insurance policy itself. Whereas life insurance was first peddled as protection for the family, it was later advertised as a form of investment, and policyholders who were once urged to be altruistic were now urged to be self-interested. But this alternation between beneficence and commercialism, Zelizer argues, is just one manifestation of the structural ambivalence created by the attempt to meet the demands of the market without violating public morality. Another manifestation of this ambivalence occurs in the role of the insurance agent, who does the dirty work of trading on death. Despised by the public and rewarded by the company only for high premium volumes, life insurance salesmen attempted to raise their status by identifying themselves first with missionaries and later with professionals.

The book is carefully researched, addresses important questions, and is

fascinating. As an answer to the question of why life insurance finally caught on, though, it is something of a mixed bag. Although I am quite willing to believe that noneconomic factors played a part and that Zelizer has made an important contribution by discussing them, I think she both understates the role of economic factors and overstates the role of values and ideologies.

My first complaint against Zelizer hinges on a somewhat picky methodological issue. Zelizer defines the phenomenon to be explained rather narrowly: she is not interested in the meteoric growth of life insurance from 1840 to 1890 but only in the first burst of growth between 1840 and 1859. Many of the possible "economic" explanations are rejected because they do not apply to this narrow band of time. For example, Zelizer argues that the purchasing power of the population was sufficiently large to make the purchase of life insurance feasible in earlier decades. Similarly, she argues that significant changes in life insurance policies were not made until the 1860s or later, too late to account for that first burst of growth. (Herman Krooss and Martin Blyn in *A History of Financial Intermediaries* [New York: Random House, 1971] disagree: whole life policies were introduced in the 1840s, endowments in the 1850s; and they cite 1846, rather than 1861, as the date for the adoption of the first nonforfeiture clause.) But when Zelizer turns to the changes in ideologies and values that she argues account for that first burst of growth, her evidence is drawn from the entire 19th century with occasional dipping into the 20th. A very rough count of Zelizer's quotations and footnotes by the date the source was published suggests that most of the debate about values and ideologies was centered in the late 1860s (and provides no substantial evidence that values opposed to insurance occurred any earlier than values favoring insurance), well outside Zelizer's target period. One particularly annoying example of this tendency to reject an economic explanation for a reason which applies equally to the noneconomic explanation is the case of industrial life insurance. Industrial life insurance was developed in 1877. So the fact that poorer people could finally buy small life insurance policies cannot be used to explain the early burst of growth in life insurance. But when Zelizer discusses how life insurance exploited the symbolic tie between money and death, one example she cites is the purchase of industrial life insurance to cover funeral expenses.

Although Zelizer admits that her evidence on value changes is drawn from the entire 19th century (p. 2), she nowhere discusses the poor match between the explanandum and the explanans. Obviously this poor fit is not entirely her fault: a selection of quotes tells us little about what proportion of the people held what views when. But I see no reason to believe that the effects of economic changes would be confined to a short period when value changes take a whole century. If economic changes have their effects so quickly, one wonders why economists are so interested in lagged variables.

One is especially uneasy with an assumption that economic factors work quickly when the economic changes being discussed are so profound. Just

by way of illustration, let me note that during the period 1816–36, a country storekeeper often did a week's business without seeing \$5 in money, that in May of 1847 a writer in *Hunt's* estimated that the money circulation per capita had multiplied by five (the quotation from *Hunt's* does not specify the period during which this increase occurred, however), and that per capita wealth in Massachusetts rose from \$100 in 1790 to \$400 in 1840 (Krooss and Blyn, pp. 47, 69, and 67). One wonders also whether the severe depression between 1836 and 1843 might not have delayed the growth of life insurance.

A final problem is that Zelizer's cultural changes are only loosely pegged to anything else. Even if we are convinced that resistance to life insurance was due to cultural factors, we have no clear idea of what caused the later changes in cultural factors. If attitudes toward speculation changed (and speculation about death was included in the bundle), a naughty voice asks, might this not have been due to the rise of financial intermediaries which made speculation possible? In general, Zelizer does not spend much time disentangling the pieces of her rather complex causal scheme, and she provides little evidence that the changes in values are *causes* of the growth of life insurance rather than ways that people explained their behavior to themselves post hoc. Of course, these problems with fitting together different kinds of explanations and assigning causal order are not unique to Zelizer. But even if it is unfair to criticize her too harshly for not solving them, it is fair to fault her for not facing them head-on.

Overall, this is a fascinating and provocative book. I wish it were about twice as long and that there were more space to discuss it.

The Ways Out: Utopian Communal Groups in an Age of Babylon. By John R. Hall. London and Boston: Routledge & Kegan Paul, 1978. Pp. x+269. \$11.95.

Richard Ofshe

University of California, Berkeley

The Ways Out is likely to have a staggering impact on its readers and no detectable impact on the profession other than this review. The most vivid description I have found of an experience similar to what I enjoyed in response to this work is recorded by Hunter S. Thompson in his classic of 20th-century prose, *Fear and Loathing in Las Vegas* (New York: Popular Library, 1975). Thompson writes lovingly of recovery from a near fatal encounter with the drug adrenochrome, which is purportedly made from the adrenal glands of a *living* human body. During the recovery phase, after total paralysis has passed and the wish that death would come abates, Thompson writes: "It was after midnight when I was finally able to talk and move around . . . but I was not free of the drug; the voltage had merely been cranked down from 220 to 110. I was a babbling nervous wreck, flapping around the room like a wild animal, pouring sweat and

unable to concentrate on any one thought for more than two or three seconds at a time" (p. 134).

Unfortunately, the experiential response induced by reading this work is not matched by the response to the scholarship contained in it, and therefore it is likely to be ignored by all save an ultraselect group within sociology. The book makes intellectual claims on a grand scale, for example, "In its theoretical concerns and methodological approach, the present study is an attempt to confront the problem of multiple realities by offering an analysis based on participant-observation which explores in comparative fashion the alternative cognitive constructions and objective economic and organizational correlates which obtain in various communal groups" (p. 9).

It tends, however, to deliver either the most obvious commentary or statements such as: "The conclusion may be briefly stated here. Quite simply, the revolutions of utopian communalists are revolutions of time. In any of diverse ways, participants in communal groups modify the diachronic subjective experience and social construction of time which prevails in the established order. The utopian communal reconstructions of time are contextualized by other cognitive assumptions about social enactment, namely, whether the world is to be 'taken-for-granted' as involving 'multiple realities,' ethically and exemplarily produced via one or another kind of socially shared value rationality, or mystically transcended via ecstasy and contemplation" (p. 17).

The best parts of the book are the descriptive passages, many of which make it seem that John Hall's research carried him *Through the Looking Glass* but, unlike Alice, into a *Furry Freak Brothers* comic book.

One day at "Free Union," an open land community, the talk was of taking the cabin structure off an old truck, converting the cabin to a dwelling, and putting a flatbed on the truck. Other more immediate concerns involved finishing the dome roofing and insulation, and making the trip into town for supplies. When it started raining, we went into town, and on returning, we spent the afternoon inside, drinking and smoking, and patching clothes. By the time the rain let up, it was too late to get much done outside, but Tex was drunk enough to want to patch the roof. He and Three Paws went up to the top of the dome, and Three Paws tied a rope around Tex's leg; let him down the curve of the dome with patches, roofing nails and a hammer, and held tight to the rope. Tex did the job and they came down for more beer: "Brother, let me tell you, you was carrying it all up there; if you'd have gone, we'd both be gone, 'cause I was already gone!" [P. 147]

The research for this book is based on participant-observation techniques. The author must be a participant observer of consummate skill. His "accidental sample" is composed of 28 groups, seven of which are singled out as the principal cases. Of those cases, Hall's only contact with the Symbionese Liberation Army was via three popular press pieces. Of the other six, he visited four—for periods of five days (one group), six days (two groups), or on numerous occasions (one group). He participated

in one group "since its inception" in 1970, although only one "household directly descendant from it exists today" (perhaps the author's household?), and lived in the seventh principal case between 1971 and 1975.

Most of the remaining 21 groups were visited during two truck trips around the United States during which the author crashed with various groups and hung out for anywhere from one day to a week. Other groups were visited before the research actually began or had members who were friends of Hall. Apparently, one of the groups studied was a folk music band about which he had "only an outsider's view of the group as performing musicians" (p. 242).

The constraints of space have limited my commentary on this book to its strong points.

The Children of Prosperity: Thirteen Modern American Communes. By Hugh Gardner. New York: St. Martin's Press, 1978. Pp. xi+281. \$14.95 (cloth); \$5.95 (paper).

Leigh Minturn

University of Colorado at Boulder

The Children of Prosperity begins with an introductory chapter on the evolution of the modern commune movement, covering the political and social forces that produced the move to communes in the early 1970s and including a description of the author's purpose. In chapters 3-13 Hugh Gardner describes each of the 13 communes in terms of the commitment mechanisms used by Rosabeth Kanter in her study of 19th-century communes (*Commitment and Community: Communes and Utopias in Sociological Perspective* [Cambridge, Mass.: Harvard University Press, 1972]). Chapter 14 analyzes the differences in commitment mechanisms between the six communes that were still operating in 1973 and the seven that had disbanded by that time. The final chapter discusses the future of the commune movement and its effect on society at large.

Chapter 1, entitled "The Evolution of the Movement," contains three sections. The author regards the postbeatnik youth culture in the early 1960s as the origin of the movement. The strains of an unpopular war and the general deterioration of city living led to disenchantment among the educated and affluent young. A naive belief that modern technology would provide easy affluence and make the work ethic obsolete led to the belief that a more equal distribution of wealth would provide a life of relative leisure for most people. The popularization of drugs and their legitimization by Timothy Leary provided another component of the value systems of communal residents. Contributions of the popular and underground press, movies, and music fed into the belief system that sparked the movement. These diverse influences are described, though not summarized.

In the fourth section of this chapter, the author states the three goals of the book—"first, to compile an accurate history of the background and formation of a reasonably representative sample of rural communal groups established over the period of 1965-70; second, to provide a comprehensive descriptive account of their social and organizational structure through the lens of a uniform analytical framework; and third, to follow through at a later period with a report of subsequent events and an assessment of each group's success or failure over time" (p. 21). This section might better have been placed at the beginning of the text. The second part of chapter 1 is devoted to the description of Kanter's commitment mechanisms—continuance, cohesion, and control. Each mechanism has subcategories: sacrifice and investment for continuance, renunciation and communion for cohesion, and mortification and transcendence for control. This scheme is described in some detail, since it forms the basis of the author's work.

The last part of chapter 1 presents the author's method. The sample is limited to rural communes in Colorado, New Mexico, California, and Oregon. Unlike most descriptive work in the social sciences, the book lists the real names of the communes and the people in them. The descriptions are drawn from the author's observations and other material published on the same communes and the movement. A more detailed account of the fieldwork is given in the appendix, "Notes on Field Work." The author visited the communes during the summer of 1970, staying in each place from two days to a week. In the first chapter the author refers to a second trip in 1973 when he determined which communes had succeeded or failed, but no mention of this trip appears in the appendix.

The bulk of the book is devoted to describing the 13 communes in the sample. Each description includes (1) a history of the commune, (2) an analysis of its commitment mechanisms, and (3) a history of the commune from 1970 to 1973. These descriptions are stylistically well written and make interesting reading. However, no attempt is made to eliminate the author's evaluations from the text. Since these opinions are usually presented without validating evidence, the objectivity of the reports is somewhat suspect.

Chapter 14, on analysis and interpretation, describes the differences between the six successful and the seven unsuccessful communes in terms of Kanter's six mechanisms. The data are presented in tables and analyzed by ρ coefficients. The text does not describe the coding system. The table in the appendix includes more than one scale per mechanism and does not present the codes for each commune. Gardner finds that only two of the mechanisms, sacrifice and renunciation, are positively related to commune success in his sample. Overall, use of commitment mechanisms fails to distinguish significantly between successful and unsuccessful communes.

The last chapter is devoted to an analysis of the "destiny and legacy of the commune movement." Here the author distinguishes between anarchistic and religious communes, and later between salvational and intellectual religious communes. This classification is not applied to the communes in the author's sample, and one wonders why it was introduced in the last

chapter rather than the first. The author's conclusion is that while the communal movement has ended, its contribution to American society is the "back-to-the-land" movement. The evidence presented for this movement is that the largest increase in population between 1970 and 1973 was in rural communities. Since causality cannot be inferred from a correlation, his conclusion seems unwarranted. Even if one assumes that the two phenomena are causally related, the rural commune movement may be a subcategory of the larger movement. Since urban communes were dropped from the sample and only four states included in the survey, the inference is based on very skimpy evidence.

This book provides the reader with some descriptive information on 13 communes. The historical analysis is better than the analysis of the future of the commune movement. The methodology and organization of the material fall short of the usual standards of scientific research.

Sex Roles, Life Styles and Childbearing: Changing Patterns in Marriage and the Family. By John H. Scanzoni. New York: Free Press, 1975. Pp. ix+259. \$12.95.

Illegitimacy, Sexuality and the Status of Women. By Derek Gill. Oxford: Basil Blackwell, 1977. Pp. xvii+362. £11.50.

Lynn Smith-Lovin
University of South Carolina

Both John Scanzoni and Derek Gill attempt the difficult and important task of relating women's childbearing behavior to their familial roles and extrafamilial status. Both have collected valuable data sets that should increase our knowledge of these topics. Unfortunately, both demonstrate also that interesting data cannot contribute much to the understanding of complex processes in the absence of a theoretically generated model specification.

The authors are aware of their need for theoretical guidance in data analysis. Scanzoni in particular, in *Sex Roles, Life Styles and Childbearing*, goes through the motions of developing a theoretical framework upon which he bases his data collection and statistical analysis. But his theory remains at the level of bivariate relationships, and the reader is soon lost in a maze of correlations. Indeed, sometimes the text reads like an element-by-element commentary on a massive correlation matrix. Although more comprehensive regression models are offered in later chapters, the theoretical discussion does not undergo parallel development.

Scanzoni intends to anchor his analysis to a utility model of fertility control; he suggests that childbearing is dependent on perceptions of the available resources, rewards, and costs associated with fertility. He measures common sociological variables such as race, father's education, mother's education, age, religion, and respondent's education and views them as

determinants of sex-role orientations. These orientations are said to represent the respondents' preferences or "tastes" for different activities. Scanzoni assumes that these preferences are established prior to marriage and are highly resistant to change. Then the background variables and sex-role orientations are related in a roughly causal way to birth intentions, current parity, and contraceptive use. Prior to the analysis, bivariate hypotheses are suggested for the relationship between each pair of variables.

The differences between Scanzoni's rough framework and traditional economic-utility models are important. Economic analysis proceeds by making some fundamental assumptions about the psychological characteristics of consumers and then by rigorously deducing hypotheses about observable behavior (see Boone A. Turchi, "Microeconomic Theories of Fertility: A Critique," *Social Forces* 54 [1975]: 107-25, for an excellent review of the model). In general, such models assume that a couple will choose a family size that will maximize its utility, taking into consideration (1) the quantity of other goods consumed by the couple, (2) an expenditure measure of the "quality" of children chosen by the parents, (3) the price index of the other goods consumed, and (4) the couple's available income. The rigor of the analysis is obtained from the precision with which testable hypotheses can be derived from stated underlying assumptions and from the ability to translate all factors into a single economic property, money. To allow the derivation of specific predictions, it is usually assumed that preference structures are homogeneous within the population, that completed family size is the equilibrium result of a lifetime optimization process (as opposed to a dynamic model that requires a new decision at each parity progression), and that child quality is a decision variable.

From this brief contrast of Scanzoni's research and the traditional economic model, one can see both the contributions and the shortcomings of his work. He inserts a bit of social reality into the utility model by allowing background factors and current life conditions to affect preferences for children and other activities; he recognizes the heterogeneity of his population and allows their tastes to vary. But by abandoning the formal nature of economic theorizing, he has lost its rigor and precision. He cannot state in advance (or even after his analysis) the manner in which social values will affect the optimization process. Only the net direction of the effect can be hypothesized—a taste for doing other things will lower the number of children wanted. Therefore, Scanzoni's analysis could more reasonably be called a study of values or attitudes, in the sociological tradition. Simply describing a birth as "incurring a familial cost" is not the same thing as employing a utility model with its functional specificity and precise deduction.

On the other hand, Scanzoni's model retains some of the undesirable aspects of microeconomic utility models. He assumes that the decision about completed family size is made early in married life and is affected by "tastes" formed firmly during this period (or earlier). He does not consider explicitly the interaction of husband and wife during this decision making or the possibility that structural barriers may not allow the couple

to implement their decisions. Indeed, the influence of later life events on sex-role orientations and childbearing behavior is considered only as a qualifying note in the discussion of results.

Scanzoni often notes the regrettable lack of longitudinal data as an explanation for the absence of a more comprehensive, dynamic model. The absence of good follow-up studies is a problem, but clear conceptualization and model specification can extract quite a bit of information from cross-sectional data. In general, Scanzoni does not make efficient use of the data he has. For example, his theoretical discussion suggests strongly that sex-role orientations are determined by family background and socialization and that these values then influence decision making about childbearing. However, my reanalysis of his correlation matrix for female respondents (table 2-5, p. 209) shows that his seven major background variables explain only an average of 8.8% of the variance in the 10 sex-role scales. Thus we do not need longitudinal data to see that sex-role orientations are produced by something other than these variables. This fact is not taken into account when Scanzoni claims the data's support for his general framework.

Similarly, one can learn a lot from cross-sectional data about the relationships between age at marriage, work before marriage, and family size at a particular point in time. But the problem must be conceptualized carefully, because the first two variables limit the years available to a couple for childbearing. Timing effects and effects on completed family size are difficult to disentangle. Scanzoni simply correlates age at marriage and premarital work with current family size and interprets the strong relationships as indicating that preferences for "individualistic rewards" lead women to defer marriage and to have fewer "familistic benefits" (children). Translating from the utility-model jargon, he interprets the relationships as indicating an effect on completed family size. Actually, since he does not control for the length of the interval available for childbearing (which is directly affected by age at marriage and indirectly affected by employment before marriage) this conclusion is questionable.

Scanzoni's data are rich, and the measurement of sex-role orientations is carefully developed. But the report is a hodge-podge of information bits, sometimes meaningful and sometimes impossible to evaluate in the absence of a comprehensive model specification. Without more precise theoretical work, Scanzoni might have made a greater contribution if he had published only a rough theoretical outline, the data collection procedures, and a complete correlation matrix. Then the reader would be free to evaluate his own model.

Gill's book on illegitimate childbearing, *Illegitimacy, Sexuality and the Status of Women*, contains three distinct studies: a study of illegitimacy levels over time, a survey of unmarried mothers, and an investigation of the educational and social careers of illegitimate children. The three are held together under the very general hypothesis that changing attitudes about sexual behavior and the status of women determine both the level of illegitimacy in a group and the consequences of illegitimate birth for the

mother and child. The "theory" here consists of a lively social history that uses literature and popular song lyrics to illustrate patterns of sexual behavior and values from Victorian times to the present. Gill then attempts to relate current social values to the results of his primarily descriptive studies.

With such a loose conceptual framework, it is not surprising that the book fails to make much progress toward Gill's stated goal, a theory of illegitimacy that would explain both the aggregate level of illegitimacy and the individual phenomena related to it—prenuptial conception, abortion, adoption, and subsequent experiences of the unmarried mother and her child. Still, so little is known in this area that a good descriptive study at the aggregate and individual levels could contribute a great deal.

Unfortunately, Gill's descriptive analysis is flawed in some respects. The most serious problem occurs in the first two chapters, where trends in the illegitimacy ratio (the number of illegitimate births per 100 live births) are analyzed for several time periods, geographical areas, and social groups. Gill concludes that a dramatic increase in illegitimacy occurred over the past two decades in England, Wales, and Scotland. He also observes a new pattern of illegitimacy in which urban areas have higher rates than rural areas, and a marked increase of illegitimacy among women under age 20 and among upper-SES women. The individual-level studies that follow in chapters 3–5 are presented as an attempt to answer the question "Why is this new pattern of illegitimate birth, associated with youth, urban living, and sophistication, evolving?"

Unfortunately, the illegitimacy ratio is such a poor measure of illegitimate fertility levels that both the general trends and the group differences that are observed are almost certainly misleading. Since it is the ratio of nonmarital births to all births, the illegitimacy ratio is drastically affected by marital status distributions and the level of marital fertility. For example, if one observed 100,000 women with a legitimate fertility rate of .20 and an illegitimate fertility rate of .05, the ratio would be .05 if 80% of the women were married but would rise to .40 if only 50% were married. Since marital status and marital fertility vary dramatically with the rural/urban and SES differences that Gill analyzes, his results are impossible to interpret.

A better measure of illegitimacy levels would be a ratio corrected for marital distributions or the number of illegitimate births per 1,000 unmarried women. Recent evidence using a measure that reflects only the degree of reproduction outside of marriage indicates that England, Wales, and Ireland experienced *no* change in the level of illegitimacy between 1966 and 1971 (Elwood Carlson, "The Dispersion of Reproduction beyond Marriage," unpublished paper). Therefore, it is likely that almost all of the fluctuations and group differences that Gill observes are due to the contamination of his measure, not to any substantively important variation in social circumstances or attitudes toward sexuality.

Gill's individual-level data on mothers and illegitimate children are more sound. However, they were collected under an epidemiological model and many of the variables will not be interesting to sociologists. Illegitimate and

legitimate pregnancies are compared on such variables as social class; mother's age, height, and weight; delays in antenatal care; and incidence of pregnancy complications. The results will be most interesting to medical sociologists concerned with health-care delivery. Later analyses of the decision whether or not to give up an illegitimate child for adoption, and analyses of the impact of illegitimacy on the child (including outcome variables such as health problems, parent's attitudes toward school and homework, child's use of leisure, occupational aspirations and educational attainment of the child, and financial difficulties in the home), will be of interest to family researchers. Almost none of the results speak directly to the central question that Gill poses: how the reproductive careers of women are influenced by attitudes toward sexual behavior and the status of women in society. His theory is not developed enough to guide his data collection and analysis.

Both Scanzoni and Gill illustrate the fact that the abstract relabeling of empirical relationships and entertaining social history cannot replace a theory that is specific enough to translate into a quantitative model. Only such a model can provide simultaneously a coherent summary of the data collected and a useful test of how well the theoretical model conforms to the observations. A reader can learn a lot about childbearing, family forms, and women's position in society from these two books, but what he learns is in rough form. One must provide one's own model and fit the empirical pieces together within that framework. Thus the authors have missed much of their opportunity to make theoretical contributions.

The Africanization of the Labor Market: Educational and Occupational Segmentation in the Cameroun. By Remi Clignet. Berkeley and Los Angeles: University of California Press, 1976. Pp. xvi+230. \$12.75.

James A. M. Elliott
Georgetown University

Remi Clignet's monograph explores the influence of formal education, ethnicity, and numerous other factors on the occupational attainment and earnings of white-collar and blue-collar workers in the modern private sector of Cameroon in the 1960s, using data from a survey conducted in 1964 and 1965 by the Cameroonian statistical and economic research service. A major purpose of *The Africanization of the Labor Market* is to confirm the existence, and measure the extent, of segmentation in the labor market and to test the validity of what Clignet calls the "principle of scale" in determining the relative importance of ascriptive versus universalistic factors (such as formal education) in explaining the income and occupational achievement of white-collar and blue-collar workers in a developing country. His ultimate aim is to draw conclusions applicable to the planning of educational investments in Cameroon and other LDCs. He also seeks to

assess the implications for the development process of a more or less speedy "Africanization" of high-level positions in modern private-sector firms.

The quantitative techniques Clignet employs include visual inspection of grouped data, stepwise multiple regression analysis, and factor analysis. His regression procedure is to fit two equations, one for blue-collar workers and one for white-collar workers, for each relationship examined. The regression results are presented in terms of "beta coefficients" (by which, presumably, he means standardized regression coefficients), and incremental contributions to R^2 of the variables successively incorporated into his regression equations by the stepwise procedure.

To summarize Clignet's findings briefly, it appears that in Cameroon at the time of the survey manual workers, compared with white-collar workers, received much lower average wages, had a much more pronounced tendency to downward job mobility, and had earnings profiles influenced somewhat less by educational investment and rather more by ethnicity and employer's characteristics. In addition, manual workers had much lower average levels of schooling. It seems reasonable to accept Clignet's principal thesis that the labor market in the modern private sector is "segmented" in that blue-collar workers have distinctly poorer lifetime earnings and occupational attainment prospects than white-collar workers and little chance of moving into white-collar work.

The labor market can also be considered as being segmented between low-wage firms and high-wage, progressive firms, which command greater worker commitment as evinced by low turnover rates. Clignet finds that firms with European employees tend to offer less prospect of promotion to high-level positions to their Cameroonian employees, but, as if in compensation for this, pay them higher wages than they would get in fully Africanized enterprises. "Africanization" (i.e., Cameroonianization) of top-level positions, Clignet suggests, may thus have as its price (for the mass of modern-sector workers) a reduction of wages. This is one of his most interesting points, and he makes much of it in his discussion of "transformationist" versus "gradualist" approaches to the Africanization of high-level positions. The causes and prevalence of this phenomenon are of obvious interest to policymakers; Clignet suggests that further research is needed to identify the enterprises which are the best candidates for encouragement to replace expatriate Europeans with Africans in high-level positions.

Another major concern of the book is the implications for educational policy—given the segmentation of the labor market along blue-collar/white-collar lines—of the tendency for the educational system's output to grow faster than the modern sector's demand for white-collar workers. One gathers that Clignet thinks Cameroon has overinvested in education relative to job-opportunity expansion, producing a situation in which school leavers increasingly experience unemployment or find blue-collar jobs, pushing the less educated to the end of the job queue or out of the modern-sector labor market altogether—without bringing any greater productivity potential to the job than their less educated competitors.

A reduction in government spending on education (the logical solution,

according to Clignet) would be politically unacceptable both to the population at large, which still views education as the prime means of social mobility, and to employers, who want to minimize their share of the costs of screening and training prospective employees. Clignet rejects a transition toward a more vocationally oriented form of education on the grounds that this would involve the schools' doing, at taxpayers' expense, what firms are almost certainly capable of doing better, and at lesser cost, themselves. This seems to leave us at an impasse.

The finding that investment in education is excessive and needs to be cut back, however, is based on shaky evidence. Clignet's regression analysis, which purports to show the unimportance of schooling as a determinant of blue-collar earnings, is unconvincing. He claims to have found that schooling, as measured by his "aggregate training score" variable, plays a less prominent role, relative to other factors such as ethnicity, for blue-collar workers than for white-collar workers in determining income and occupational attainment. The proportion of variance accounted for by schooling and other performance-oriented variables is somewhat greater in the white-collar regression in both cases. But how great the difference must be to be statistically significant is not made clear—nor is the wisdom of relying on the arbitrary stepwise regression procedure rather than theoretical considerations to decide on the variables to be incorporated into the regression equations finally fitted.

One aspect of Clignet's regression results which seems to have escaped his attention, and which at first glance seems to confirm the unimportance of schooling as a determinant of earnings, is the remarkably small effect additional schooling seems to have on income *within* each group, according to the beta coefficients for the schooling variable in his regression equations on pages 132–33. Although this is not necessarily inconsistent with a situation in which the primary payoff of education may be its tendency (for which Clignet supplies evidence) to increase the individual's probability of entering the white-collar labor market with its higher lifetime earnings, it is hard to believe that additional schooling is almost irrelevant as a determinant of earnings within market segments, especially in view of Clignet's own comments on this subject (p. 116) and the evidence of his grouped data tables 26–29 (pp. 117–23).

One possible explanation is that the regression results may contain typographical errors. A second is that Clignet's regression procedures are virtually guaranteed to produce downward-biased estimates of the education coefficients. Running separate regressions for the high-earning white-collar population and the low-earning blue-collar population, for example, is likely to produce truncated regression bias. Thus, although some readers more alert than Clignet himself to the apparent implications of his regression results may be inclined to take them as evidence that investment in education in Cameroon has been excessive or misguided or both, such a conclusion would not be warranted.

While ingenious, Clignet's attempt to infer a decline over time in the social return to education, based on an inspection of grouped data tables on

job-mobility experience of older and younger workers with varying amounts of schooling (pp. 104–6), is also ultimately unconvincing. All things considered, his analysis seems to paint an unduly dismal picture of the long-run developmental contribution of Cameroon's relatively heavy commitment to education.

Clignet's awkward prose style and unconventional use of English put heavy demands on the reader's patience. For instance, he consistently refers to "the Cameroun" (a carry-over in both phrasing and spelling from the French) rather than "Cameroon." Other departures from conventional English usage could be cited. The publisher's failure to eliminate them forms part of a pattern: the pages in the final chapter of my reviewer's copy are inserted out of order, and the title printed on the dust jacket does not quite agree with that on the title page (the latter is used in this review).

The Social Bond: An Investigation into the Bases of Law-Abidingness. Vol. 1: *Antecedents of the Social Bond: The Phylogeny of Sociality.* Vol. 2: *Antecedents of the Social Bond: The Ontogeny of Sociality.* By Werner Stark. New York: Fordham University Press, 1976, 1978. Pp. ix+229; viii+242. \$15.00 (cloth); \$7.50 (paper) each volume.

Forrest Dill

University of California, Davis

These are the first two volumes of a projected six-volume effort launched by Werner Stark to remap territory first navigated by W. G. Sumner in *Folkways* (Boston: Ginn, 1906) and E. A. Ross in *Social Control* (New York: Macmillan, 1901). On this great voyage he plans to update those famous studies by synthesizing findings from sociology and other disciplines that bear on the problem of society, that is, what accounts for coherence and order in human populations? Thus the journey is not intended to yield any new discoveries; Stark acknowledges in his preface that he has been guided by Durkheimian and Parsonian conceptions of social integration in charting his course. Instead the goal is to produce something like an encyclopedia of knowledge about what the author refers to as "sociality"—the potential with which humans are born that, when realized, transforms self-centered individuals into sympathetic cooperators and cements them together in integrated societies.

The first volume, *Antecedents of the Social Bond: The Phylogeny of Sociality*, starts with a long chapter containing a detailed examination of possible analogies between animal society and human society. Sociologists are likely to find this discussion the most valuable part of the two volumes. What, Stark asks, do we learn by comparing human social systems, on the one hand, with those of insects, birds, fishes, and mammals, on the other? In particular, is there any scientific evidence for natural (i.e., instinctive or genetic) bases of order among populations of *Homo sapiens*? The answer

Stark supplies, prefigured in the preface, is that human beings achieve order not simply by acting as vehicles of biological imperatives but by harnessing those imperatives to cultural patterns. Humans are animals, but they are unique in having the capacity to transcend their animal propensities. Behavior among nonhuman animals is entirely determined by physical drives, sovereign natural forces that compel feeding and reproductive activity and the derivative forms of social life to which these instincts give rise (flocking, herding, presenting collective defenses against enemies, caring for young, mutual grooming, and so on). Among humans, however, social life depends heavily on "nonnatural" forces of culture which reduce and control the influence of organic drives on behavior. Therefore, parallels between animal and human societies—for example, territoriality and hierarchy—turn out to be spurious. That is, structures of animal societies are explainable in terms of instinctual demands and environmental adaptations. Human social structures are principally cultural achievements.

The author devotes much of his review of the literature of ethology to combating errors committed by anthropomorphists, whose sentimentalism causes them to interpret animal behavior in human terms, and sociological instinctualists, whose yearning for the prestige of natural science leads them to exaggerate grossly the significance of inherited factors for explaining human social behavior. But the discussion is not limited to that objective, for Stark also addresses a number of questions that interest students of contemporary theoretical biology: whether the process of natural selection operates in gene pools belonging to groups of animals or to individual animals, whether evolution is emergent (involving linear movement along a single dimension) or divergent (branching out on different paths), and—in a postscript to the first volume—whether "altruism" in nonhuman species is generically similar to "altruism" in human beings, resting on the same foundation.

In the last two chapters of the first volume, Stark begins more affirmatively to develop his thesis concerning the basis of human sociality. Using data from accounts of human isolation (feral children), utopian communities, lynch mobs, and police strikes, the second chapter shows that the recrudescence of animal tendencies in human behavior is a threat that even the most technologically advanced societies are unable to keep under control at all times. This discussion does not analyze the conditions under which culture is likely to become attenuated and social control to break down; instead, the purpose is to demonstrate that animality—behavior rooted in bodily based urges—remains alive in the depths of modern man. The third chapter considers the origins of the social bond among humans by examining anthropological descriptions of "primal" societies. Stark draws heavily on accounts of arctic Eskimo and aboriginal Australian life for materials illustrating how the least developed societies manage to moralize the conduct of individuals and thereby to exert social control over greed, sexuality, and aggression.

The second volume, *Antecedents of the Social Bond: The Ontogeny of Sociality*, deals with socialization, the process which liberates human beings

from domination by somatic impulses and links individuals to each other through internalization of common norms and values. Like the first, this book reaches beyond sociology—in this case, to psychology—for insight into the problem of human sociability. The main argument is that socialization entails substantial conflict between the individual being socialized, who enters the world as a bundle of organic drives seeking release, and agents of socialization, who desire to put limits on the time, place, and manner of expression of those drives. Stark maintains that this original and basic antagonism between bodily based impulses and cultural demands persists unresolved into and sometimes beyond the first seven years of life. From this perspective he criticizes Cooley, Mead, and Piaget for having underestimated the strength of instinctual urges and overestimated the power of social influences in the development of the self. Yet he does not agree entirely with Freud, whom he finds in error for having portrayed the id as being unmodifiable. Stark further distinguishes his position from that of Freud in the latter part of the second volume, where he takes up the question of human diversity. Reviewing the writings of leading culture-and-personality theorists (Erikson, Gorer, Honigmann, Kardiner), he advances a view of the socialization process that attaches greater significance to cultural patterns which last throughout the lifetimes of individuals than to experiences of infancy and early childhood in explaining adult character.

The author subtitles his study "An Investigation into the Bases of Law-Abidingness." It is too early to know what kind of contribution this work will make to our understanding of deviant behavior and social control. The first two volumes contain only the suggestions that deviance is a product of incomplete socialization, defective external control, and weakened culture. The promised third volume will deal directly with the phenomena of deviance, social control, and law. Perhaps Stark will produce a novel synthesis of findings in that volume. In any case, the third and subsequent volumes, like the first two, can be expected to offer valuable treatments of developments within and outside the discipline of sociology that will help bring into clearer focus some of the issues of central concern to all students of society. Whether the series as a whole is marked for the stature of the classic works that the author has set out to emulate, only time will tell.

Cultural Conceptions and Mental Illness: A Comparison of Germany and America. By John Marshall Townsend. Chicago: University of Chicago Press, 1978. Pp. xvi+150. \$11.00.

George Becker
Vanderbilt University

The dominant conception of mental disorders as manifestations of illness has come under extensive criticism over the past 15 years. A growing number of social scientists and clinicians have questioned the theoretical and

empirical adequacy of the medical model as well as its therapeutic effectiveness. A unifying theme of these critics is the assertion that the behaviors commonly identified as symptomatic of illness are in reality learned adaptations to the social and physical environment—as such, they cannot be understood by reference to disease processes. The challenge to the clinical model has unleashed polemics in which each side has charged the other with misunderstanding, oversimplification, and willful distortion of facts. In place of a new consensus or synthesis of the two perspectives, the recriminations have led to an impasse and polarization on relevant issues.

John Marshall Townsend's *Cultural Conceptions and Mental Illness* has as one primary goal the resolution of this impasse. Even though the proposed resolution is less than totally successful, Townsend's work is a carefully conducted empirical study which, by critical reference to other works, employs its findings to develop a potentially viable compromise position. Although this study focuses primarily on what Townsend calls a social-role approach, the author does not succumb to the temptation of seeking to vindicate a sociological interpretation at the expense of the clinical viewpoint. What results is a balanced attempt to explore some of the tenets and implicit assumptions of both schools that gave rise to distortions and misunderstandings.

The comparison of matched samples of German and American mental patients, students, and clinicians provides Townsend with evidence that he views as simultaneously supportive of and detrimental to the labeling paradigm. Consistent with the labeling position is the finding of significant intercultural differences in the conception of mental disorders. Townsend concludes, in support of the labeling framework, that these conceptual differences manifest themselves in culturally specific attitudes and coping techniques on the part of patients. Unsupported by his findings, however, is Thomas Scheff's labeling assertion that such stereotypes of insanity as wild, unpredictable, and dangerous behavior act as a guide for symptom formation. Also, consistent with findings of other empirical works, the patients in Townsend's study denied that they were mentally ill and failed to identify with patients so perceived. This refusal to adopt a deviant self-concept leads the author to question the applicability of Erving Goffman's conversion hypothesis to mental hospitals.

As with the labeling perspective, Townsend perceives both validity and distortion among the premises of the medical model. He concurs, for example, that a wide range of empirical evidence supports the central tenet of the clinical position that major psychoses are encountered universally and are generally identified as aberrant in every society. This pronounced focus on the more extreme forms of mental disorders, however, has tended to blind advocates of the clinical model to the empirically verifiable facts that diagnostic labels are a partially cultural product, are often arbitrarily applied, and occasionally serve as a means of social control. Labeling theorists, of course, have given explicit attention to these concerns, but they have done so, Townsend argues, by a one-sided approach which has failed to address the importance of physiological factors productive of mental disturbances.

To Townsend, therefore, what is central to the impasse between the schools is their adherence to a false dichotomy between biology and social learning. Inevitably, the author argues, the result of this dichotomization is the employment of different populations and phenomena by each school to buttress its arguments. The selection and manipulation of evidence to support an entrenched and exclusive position is, of course, a practice all too commonly encountered. And it is often attended by a great expenditure of energy on questions that do not promote clarity and accuracy. In this instance, Townsend notes that the controversy has centered overwhelmingly on "which approach can best account for the majority of the evidence and which best explains 'what typically happens?'" (p. 89). He believes the misplaced concern for this question has led to semantic disagreements distracting attention from more meaningful empirical issues.

Townsend perceives the resolution of the existing impasse as (1) recognizing that each school uses different populations and has different foci and (2) advancing a psychophysiological conception of social role, one that is capable of incorporating evidence from both perspectives. Such a compromise posture is justified, we are told, by the empirically verifiable fact that mental disturbances involve both biological and social-role processes. And conversely, the author argues, social roles are intimately tied to organismic processes.

Cultural Conceptions and Mental Illness makes an important contribution that scholars of mental illness and deviant behavior cannot afford to ignore. Addressing a debate that has often tended to generate more heat than light, it identifies perceptively many of the misunderstandings, distortions, and implied assumptions that have usually been obscured in the course of the polemics. It demonstrates, moreover, that resolution of the existing impasse is not precluded on empirical or theoretical grounds. The usefulness of a psychophysiological conception of role in resolving this impasse is difficult to assess, for the author's treatment of it tends to be vague, speculative, and impressionistic. It appears, nevertheless, that Townsend has sketched in general terms the outlines of a potentially viable framework deserving of attention in the resolution of this debate.

So Far Disordered in Mind: Insanity in California, 1870-1930. By Richard W. Fox. Berkeley and Los Angeles: University of California Press, 1978. Pp. xvi+204. \$10.00.

R. A. Whitfield

Indian Nations Council of Governments

Few of my friends could resist the temptation to tell me that volume 2 of this work, covering the years since 1930, will undoubtedly be a much longer book. If so, I will look forward to it. While Richard Fox has written "yet another" of the many works on the social context and concept of

mental illness, *So Far Disordered in Mind* is refreshingly free from the usual preoccupation with the validity of the labeling perspective. The methodology is not novel, but it turns out that transcending the battle over the medical model of mental "diseases" leaves lots of room in a short book for an original and intriguing account of the mechanisms and the ideology of social control. It is not at all a left-handed compliment to say that, when authors end their papers with the standard plea for more research, this is the kind of scholarship they ought to have in mind.

This is a book about the "rationalization" (as Weber meant the word) of the state's role in the social control of mental illness described from two perspectives, that of the patient and that of the mental health profession. Drawing from a population of 12,150 persons declared insane in San Francisco from May 1906 to May 1929, Fox analyzes a systematic sample of 1,229 case histories in order to focus on the types of persons officially judged insane, their behavior, and their accusers both inside and outside the psychiatric bureaucracy. The picture emerging from this analysis could have come from more contemporary statistics: Fox finds very high rates of commitment for adult males, the unmarried and the widowed, and blue-collar workers. But the principal virtue of the book lies in linking this social pattern to the politics of the institutional and professional network of administrators, the administrators' desire for social recognition and, not incidentally, for larger appropriations from Sacramento.

The social history of insanity in California from 1870 to 1930 shows a striking differentiation of psychiatric institutions as well as a slow extension of civil rights to the insane. Indeed, California, as in so many other ways, was at the cutting edge of the movement. From the 1870s to the 1920s California showed a zeal for incarceration which produced the highest rate of commitments in the nation—largely resulting, Fox argues, from its refusal to establish a state almshouse for the destitute. As a consequence, administrative considerations of overcrowding led to the establishment in California of community psychiatric wards. And from a belief in the genetic origin of mental illness (and out of sympathy for future generations of state taxpayers) emerged an even more draconian solution, the sterilization of the insane.

The community treatment wards served the dual purpose of establishing an interim facility to screen out those not "truly insane" and assuaging public fears of being railroaded to the state hospital. Short-term voluntary admissions and "preventive work" began to be promoted, saving the hopeless cases for the large state institutions. The local treatment centers also made it possible for doctors to seek the legal detainment of "peculiar individuals" before they became chronically ill—and to do so without the need to obtain court approval.

Fox gives careful scrutiny to the patients and their social relations with relatives and friends, doctors and police. He has written an almost nostalgic account of the clash of cultures, social mores, and personal habits placed against a San Francisco backdrop of "primitive psychology" and rampant ethnic, sexual, and age discrimination. Unfortunately, 50 years of histori-

cal distance will not provide much comfort to anyone professionally or personally familiar with the current status and treatment of mental illness; many case histories Fox details could have come from the files on last night's admissions to the city psychiatric ward.

Even so, Fox explicitly avoids portraying his subjects as innocent victims of some elite conspiracy.

The temptation is strong to reduce the history of social welfare and of the "helping professions" to a morality play pitting the malignant "controllers" against the poor deviants who sought only to march to the beat of their own drummer. The best way to resist that temptation is to acknowledge that . . . local authorities and the families of the insane themselves continued to use the process for their own ends. For the insane were a threat not to social order in the abstract, but in many cases to the "public order" of their neighborhoods and the tranquility and financial survival of their families. [P. 163]

Fox presents ample evidence to document the operation of the convenience function of civil commitment—"the use of the commitment statute to relieve society or the family 'of the trouble of accommodating persons who, though not dangerous, are bothersome'" (p. 138).

Paralleling the differentiation of psychiatric institutions, the concept of mental illness became increasingly distinct from the concept of insanity during this period and, ultimately, assumed a different kind of utility altogether. "The 'mentally ill' might often, despite their affliction, be functioning members of the community. . . . The 'insane,' on the other hand, were all the more clearly recognized as disruptive, bothersome, unproductive . . . in need both of corrective treatment and detention" (p. 180). Using historical records, Fox traces skillfully the growth and the extension of interests of the mental health profession based on "the mounting conviction that minor mental disorders were nearly universal and that virtually anyone might require professional therapy at one time or another" (p. xi). This conception of mental illness, which now seems largely to have replaced moral and spiritual explanations of deviant behavior, is one of the most significant ideas of our time.

Race Relations in Chicago: Second Survey: 1975. By George Surgeon, Judith Mayo, and Donald J. Bogue. Chicago: University of Chicago, Community and Family Study Center, 1976. Pp. viii+182. \$3.00 (paper).

Morton Weinfeld
McGill University

Race Relations in Chicago continues the tradition of the study of Chicago as a laboratory of American pluralism. It consists of four distinct chapters, each dealing with one aspect of contemporary race relations in Chicago. All the chapters share the same data base: a survey of a random sample

of 299 residents of Chicago, 229 white and 70 black, conducted in 1975 by the Community and Family Study Center of the University of Chicago. The book is a report by "graduate students and the staff of the Community and Family Study Center" and covers only one item in a larger research effort; thus "it was possible to devote only limited funds toward it" (p. vii). The book suffers as a result.

The first chapter, by George Surgeon, describes white racial attitudes. The value of this chapter, like that of the others, lies mainly in the clear presentation of data, in the form of frequency distributions of responses to questionnaire items. Some of the findings are interesting: the pervasiveness of unfavorable stereotypes about blacks held by whites; the lack of any relationship (in contrast to the findings of Thomas Pettigrew in *Racially Separate or Together* [New York: McGraw-Hill, 1971]) between opposition to busing and opposition to residential integration; the decline since the 1950s in the belief in innate black intellectual equality (attributed perhaps naively to the influence of the writings of Jensen and Shockley); the high degree of prejudice exhibited by the 53 "East European Christian," or Slavic, respondents, compared with other white respondents, even with appropriate controls. Apart from these highlights the author concedes that "traditional relationships between demographic, social structural and contextual variables and racial opinions have been generally replicated" (p. 73).

In the conclusion of the chapter Surgeon attempts to place these findings in a historical context through comparison with NORC data from 1970 to 1975. Asserting a decline in the state of race relations in this period, the author cites "stagflation" and "relative deprivation," among other factors, as possible explanations. This section is speculative and unrelated to the data presentation which preceded it.

In the second chapter, Judith Mayo describes racial differences in the perceptions of the poor in Chicago. Blacks are more liberal in their attitudes about welfare, while each race believes the other is the major beneficiary of welfare programs, specifically AFDC. Finally, it was found that holders of a variety of negative racial stereotypes about blacks are also more likely to hold negative attitudes about welfare in general and black welfare recipients in particular. There is little effort to link the findings to theory or other empirical work.

The third chapter, by Donald Bogue, examines black opinion about the state of race relations in Chicago. To a large degree, the data overlap those presented in the first chapter, in which white attitudes were generally contrasted with black attitudes. In the light of current debates and of polls claiming that a majority of blacks oppose affirmative action programs, it is interesting to find that 86% of the black respondents agree that companies "should be given a quota of black workers that must be employed in good jobs." Among whites, 70% are opposed. Yet a general review of black responses leads Bogue to conclude that there is some ground for optimism (a tone which contrasts with Surgeon's more pessimistic conclusions based on the same data). He claims that fewer blacks are

demonstrating a "slave mentality" (p. 145) and that the opinions of whites are largely "conciliatory" (p. 148); both conclusions augur well for the future of race relations.

In the absence of clear patterns in the data, Bogue posits a split among the black respondents, between those who believe discrimination is "alive and thriving" and those who believe it is now "dead or almost dead" (p. 149). These two groups do not differ in age or educational attainment. This raises the (unproved) possibility of a developing attitudinal schism within the middle class. Such a condition, along with the increased salience of class differences within the black community, as claimed by William Julius Wilson and debated by others, may make it increasingly difficult to generalize about the state of "black America." There may be growing variance between and within black social classes.

The final chapter, also by Bogue, looks at racial attitudes on school-related questions in Chicago. Perhaps the major finding here is that opposition to busing involves factors not related to race. For example, "55 percent of those who would permit their child to marry a black person nevertheless opposed busing" (p. 180). There are low correlations between opposition to busing and various attitudes on school issues, such as attitudes on the integration of faculties and the integration of student bodies. This finding will support those who claim that opposition to busing reflects more than bigotry.

This is a volume in search of a genre. In style, it lies somewhere between a scholarly monograph and a research report. There is little attempt to develop any integrating theme in the four chapters, as is evident in the lack of either an introductory or a concluding chapter. The reader is bombarded with a barrage of findings, in the form of tables and several ordinal measures of association. The literature reviews vary in quality across the chapters, as does the commitment to the clear formulation or testing of hypotheses.

Methodological problems are posed by the small sample of blacks, which leads to small cell sizes in some of the tables, particularly when control variables are introduced. The sample also has many more female than male respondents, for both races. Apart from introducing a possible sex bias, this may have contributed to the small subsamples of the employed (120 for whites, 31 for blacks) of whom some key questions were asked, thus limiting the numbers of statistical controls which could be introduced in the analysis. In addition, the authors do not explore the possible role of the specific Chicago context in determining their findings and perhaps limiting their generalizability.

In short, *Race Relations in Chicago* lacks coherence; the individual chapters are short on theory and structure while crammed with findings. To be sure, many of these are of interest and represent bits of information to be integrated carefully with other data to develop a broad picture of the contemporary state of race relations in the United States. Yet the whole is far less than the sum of the parts.

Images of Law. By Zenon Bankowski and Geoff Mungham. London and Boston: Routledge & Kegan Paul, 1976. Pp. xiii+178. \$7.75.

Philip S. C. Lewis

All Souls College, Oxford

This book is avowedly polemical; as a result, it will probably not have the readership it deserves. Although *Images of Law* is written by a sociologist and a lecturer in the philosophy of law, the concluding apostrophe "Seize the time" (in capitals) is addressed to the political defendant in the political trial and advises him not to join in a legal mode of argument or accept the authority of law or the court. This is, however, a practical consequence of what Zenon Bankowski and Geoff Mungham have to say. The title of the book refers to the authors' intention to "counterpoint the images of freedom that the law raises with their reality, enslavement" (p. xii). Since they see the purposes of law as being to legitimate the authoritarian structures of a capitalist society and to impose a view of the world which will restrain men's efforts to choose and associate freely, they see lawyers and teachers of law as furthering these purposes; even such enterprises as legal services, or sociolegal studies, undertaken in order to change the relationships of the legal system and legal studies with society, are likely to do no more than make the law more acceptable and so more dominating. It is only the political defendant in the political trial (here they draw on interesting British material from the early seventies) who is likely to be able to try to bring into the courtroom the authors' alternative view of reality (which they give only in outline). Here time has done them a disservice: recent court challenges have pertained more to the way in which state authority has been exercised than to its existence.

This review feels like a replay of the liberal academic position of the late sixties: it would be wrong for those who do not accept the authors' views to write them off as outdated and illiberal, even though the authors take the possibilities of "barbarism" lightly (p. 30). But they address, rather than ignore, the cumulative effects of a divorce of legal reasoning and professional attitudes from the interests they are supposed to serve, the contingent nature of the definition of a problem as legal, the impact of public funding on lawyers and their self-interest in seeking extensions of legal services, and the ways in which the professionalization of lawyers and the construction of court proceedings contribute to the virtual elimination of the defendant as a participant in criminal proceedings. On the whole, they draw their illustrations from others, and the argument is not always strict; thus they exaggerate the powers of lawyers to define a problem as legal (pp. 40 ff.), and their argument is only minimally supported by references to the passage of drug legislation or an obscenity campaign in England.

Chapter 3 should be widely read, since it contains an account of the introduction in a British city of a scheme whereby solicitors could be

present at court and advise the unrepresented and, if necessary, appear for them. The research, carried out by Mungham and Philip Thomas, shows in great detail how criminal work was distributed and controlled by an informal network and demonstrates that the motive for most of the participants in the scheme, introduced as being "in the public interest," was their own interest in breaking the existing monopoly and securing a share in this work.

Recently the Royal Commission on Legal Services, in a comment on this point, has said: "If this is so, it is no discredit to self-employed people who must find work in order to earn a living" (Final Report of the Royal Commission on Legal Services, London, H.M.S.O., 1979, Cmnd. 7648, vol. 1, p. 93). When an occupation claims the privileges of a profession because its members act in the public interest, how does the self-interest of the members, whether in status or financial reward, relate to the public-interest principles on which the occupation relies? The authors do not go into this question, which seems not to have been discussed in the sociological literature, however much skepticism is expressed as to the public-interest claims. They are not much harder on private sector lawyers than they are on those who have gone into the field of sociolegal studies: "It is the search by people who are primarily legal academics for new ways to survive in an increasingly competitive market-place" (p. 3). In my view, this is untrue and unfair; it is certainly not supported by evidence. Some books ask the right questions, or some of them, but alienate the reader by the way they phrase or answer them: this is a prime example.

Loners, Losers, and Lovers: Elderly Tenants in a Slum Hotel. By Joyce Stephens. Seattle: University of Washington Press, 1976. Pp. xix+118. \$8.95.

Daniel R. Krause
Roosevelt University

Loners, Losers, and Lovers, a dissertation project by Joyce Stephens, is essentially a "descriptive analysis of the social world of the aged SRO [single-room-occupancy] tenant" (p. xvi). The analyses included deal with, among other topics, the processes of adaptation of elderly hotel residents, their life-styles, and the nature of the social relationships which emerge within this rather discouraging environment. While parts of the book are interesting and the analyses occasionally intuitive, it is, on the whole, a work of limited value to the gerontologist; and it will not be much more useful to those who seek only a better understanding of America's aged population, for the book has too many shortcomings.

One significant weakness, and a source of considerable irritation to me at least, lies in Stephens's apparent lack of familiarity with much of the

relevant literature. We are thus confronted with a persistent series of rather questionable assertions. For example, there is some discussion of the nature of family relationships, and Stephens states that "the weakness of kinship ties is, in fact, characteristic of the elderly in America" (p. 82). She overlooks the fact that a number of researchers have arrived at just the opposite conclusion, finding rather strong ties between the elderly and their families. It may well be true that, as with Stephens's sample population, most older people do not choose to live with their children, or vice versa, but this is not sufficient evidence for concluding that family bonds are weak or absent. Ethel Shanas reported that, despite living alone or with a spouse, some 90% of the elderly people in her sample said that they had seen at least one of their children during the preceding month (*Older People in Three Industrial Societies* [New York: Atherton, 1968]).

Perhaps an argument can be made that among certain groups there is a definite weakness in family ties. But it is also possible that such a weakness can be traced, not to the age of particular members, but to the distinctive characteristics in the institution of the American family. The emphasis in this country on independence, on privacy, on being master of one's own destiny—these traits affect all members of the family in their relationships with one another, and the result might well be termed "weak" family ties. But it is incorrect to single out the elderly in any analysis of this situation; to do so presents a distorted picture of the elderly in the family structure. A weakness in family ties is a problem for the entire society; the elderly are simply in the unfortunate position of being more susceptible to effects of the situation.

The point here is that Stephens does not discuss and develop such matters. This pattern is evident throughout the book and weakens considerably its potential argument.

One should also, even in a brief review, discuss the methodology utilized, namely, symbolic interactionism. Stephens's frequent defensive postures with the technique notwithstanding, it is fair to say that most sociologists have finally moved beyond the issue of "soft" versus "hard" research methods. But this does not mean that the use of field methods exempts one from basic requirements: clear and precise conceptualization; adequate, reliable, and definitive data; and logical and complete analyses are the *sine qua non* of scientific research no matter what the technique. Stephens provides a methodological appendix containing a description of how symbolic interactionism should be implemented, a description most of us could live with. Yet in the text the reader is assaulted with rather offhand summations such as "I encountered no . . .," "there were few . . .," "occasionally seek out. . .," etc. Since she apparently has a good understanding of the technique, I attribute the rather casual analyses to writing style.

The author's prose moves from verbosity to banality. A few illustrations will convey this far better than I:

In the anonymity of an inner-city school, absolute privacy and a high degree of personal freedom can be relished. [P. 9]

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